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ROY WALDO MINER

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SECTION OF GEOLOGY AND MINERALOGY

A REVIEW OF THE DEEP APPALACHIAN STRUCTURAL BASIN*

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Introduction

This article is planned as a broad review of the character and problems of the deep Appalachian structural basin, as that term is used by the oil and gas geologist of the eastern United States. Although the region is one of the most familiar and well-studied geologic areas in the world, there are all too few places where its many facets and innumerable problems are considered under a single heading. Indeed, there are all too few geologists who concern themselves with the area as a whole. It is thought, therefore, that a brief review may be in order; and, although this writer brings no special talents nor any new interpretations to this task, he draws, nonetheless, upon 35 years of geologic experience in the region and on a very keen interest in the area as a whole.

Presented without footnote references, this general summary draws liberally from a wide bibliography of published material and upon various symposia, conferences, field trips, and other sources that do not appear in the published literature. It is hoped that the paper will call to mind some of the interesting aspects of the region, particularly those relating to its production of oil and gas, that deserve better treatment than this brief review can provide.

Definition

As described herein, the Appalachian structural basin is a broad infolded tract that extends westward from the highlands of southeastern New York, New Jersey, and the Blue Ridge area of Pennsylvania, Maryland, and the Virginias, to the axis of the Cincinnati-Algonquin geanticline of western Ohio and southern Ontario. Its northeastern end flanks the Adirondack dome as well as the Frontenac axis that crosses Saint Lawrence River at the Thousand Islands. On the south, the basin is overlapped by Mesozoic and Cenozoic strata in northwestern Alabama and northeastern Mississippi. The deep portion, specifically to be re-

*This paper, illustrated with lantern slides, was presented at a meeting of the Section on November 1, 1954.

This paper partly parallels and partly draws upon a paper entitled "Some Properties of Deep Appalachian Basin," presented on June 17, 1954 at the White Sulphur Springs meeting of the Eastern District, American Petroleum Institute, Division of Production; and a paper entitled "Some Problems of the Deep Appalachian Basin," presented on October 8, 1954 at the Marietta meeting of the Appalachian Geological Society.

viewed here, lies primarily in Pennsylvania, southern New York, western Maryland, eastern Ohio, and the two V[illegible]rginias.

The thalweg or central axis of this basin, as now revealed by its deepest trough, extends southwestward from Centre County, Pennsylvania, through Garrett County, Maryland, into Preston, Webster, Nicholas, and Fayette counties, West Virginia, and thence with more westerly strike into Pike County of eastern Kentucky. Between Centre and Webster counties, the floor of the trough sags more than 20,000 feet below sea level. From this great depth its basement gradually rises westward to stand well above sea level along the crest of the Cincinnati anticline. Eastward from its center, the floor of the basin rises steeply to the folded Valley and Ridge belt of eastern Pennsylvania, central Maryland, eastern West Virginia, and Virginia, where accessory structures have thoroughly crumpled its eastern rim.

Whereas the west flank of the basin is generally accepted to be the Cincinnati anticline, its eastern boundary is variously defined. Some geologists regard the Appalachian structural front as the eastern margin of the Appalachian basin, for this topographic and structural boundary separates physiographic regions of contrasting character. Many folds west of the front, however, are no less complex or intense than those east of the front, and there is no stratigraphic boundary that holds this location. The author regards not only the Valley and Ridge region of the folded Appalachian Mountains but also the line of the Great Valley within what he considers to be the structural basin, for he treats the Blue Ridge and its continuations as the actual eastern limits of the region being described.

Thus the Appalachian basin of this discussion is the entire territory between the Blue Ridge and the Cincinnati arch, constituting a great northeast-trending structural depression whose eastern portion has been intimately corrugated to form the familiar Appalachian mountains. Most of its central portion is only gently folded, and its western flank is a tectonic part of the eastern interior region.

For the western and central portions of the basin, all regional structures are broad and gentle, but a faint series of secondary folds has been superimposed upon the major structural elements. Toward the east, these secondary folds become larger, deeper, and more linear; the amplitude of folding increases and, without change of form save for increased intensity, the folds grade directly into the major structures of the Ridge and Valley belt.

The basin has been variously named. Kay uses the term "Allegheny synclinorium," and restricts its eastern limit to the Appalachian front. Lafferty and others use "Appalachian geosyncline," but this term permits confusion with the ancient sedimentary basin that was destroyed at the close of the Paleozoic Era. Until some better term is available, and particularly in any discussion that treats its oil and gas possi-

bilities, the phrase "Appalachian Basin" is unambiguous and appropriate, while the term "geosyncline" seems confusing in any such connection.

Whatever the details of its earlier or subsequent history, the region has been a coherent earth segment since mid-Ordovician times, when the Cincinnati geanticline was first uplifted—probably as two domes that became confluent in one arch in early middle Silurian time. This broad arch stood intermittently above sea level during many epochs of the Paleozoic, and acted to block the westward dissemination of clastic material from the eastern highlands, as well as to limit the intermingling of fauna on its opposite sides. Thus the Cincinnati arch is one of the few features common to both the depositional basin in which the Paleozoic rocks were deposited, and the structural basin in which they now take architectural position. Otherwise, save for the still uncertain connection between the filling of a geosyncline, its collapse and ultimate upfolding, there is only a fortuitous relation between the former basin of deposition and the structural basin of our present interest. Thus, however much they coincide in location, there is an important distinction between the geosyncline of the Paleozoic and the structural basin of today, and it is imperative not to confuse the two basins.

The Internal Basinal Structure

The central axis of the basin floor—represented by the Pre-Cambrian crystalline basement—separates it into two distinct portions that possess somewhat different structural aspects (see FIGURE 1). The west half of the basin is essentially monoclinal with an eastward or inward dip toward the central axis. Because of thickness convergence, the upper rock dips are much less than the slope of the crystalline floor, the dip in younger sediments being 50 to 75 feet per mile. Subsidiary folds are low and inconspicuous and lie subparallel to the Cincinnati axis. In the east half of the basin, which passes into what is usually called the foreland tract of Appalachian folding, most secondary structures are strongly developed and grade regularly east into the great folds of the Valley and Ridge belt. Inasmuch as the amplitude of these eastern structures exceeds the slope of the true flank of the trough, the folds largely obscure the inward or westward dip toward the main synclinal axis. Indeed one easily forgets that the regional dip, even in the tightly compressed folds of the Ridge and Valley belt, is westward into the Appalachian basin trough.

Had the basin been filled with the rocks of a single formation, its internal structure would be that of a simple pile, shaped by the bounding mold of the basin basement. Instead, as the basin contains 30 to 40 separate rock formations, not to list all of its many smaller units, the basinal structure is differently reflected in each of these units.

This situation is complicated by two factors. In the first place, each

of these formations had its own area of distribution, each was adjusted to a different depression on the geosynclinal floor, and each maintained its individual shore zone or boundary line. In the second place, most of these formations thicken eastward, where the isostatic (or other) settling under the depositional load followed a different pattern for each major rock unit.

The result is revealed with a series of structural contours constructed on different rock horizons in the structural basin (see FIGURE 2). If such a series is drawn showing the –15,000-foot contour on the Pre-Cambrian floor; the –7,500-foot contour on the Tuscarora sandstone; the –1,000-foot contour on the Berea sandstone, and the +500-foot contour on the Pittsburgh coal, it becomes apparent that the central trough does not rise vertically through the contents of the basin, but rather shifts westward with each younger bed, the axis of the Permian lying 50 miles west of the deep Cambrian trough.

This shift of the axis with depth is important in considering the present oil and gas possibilities of the different stratigraphic units, for the point where any individual formation can be expected to possess a present westward updip, must always lie west or northwest of the basinal

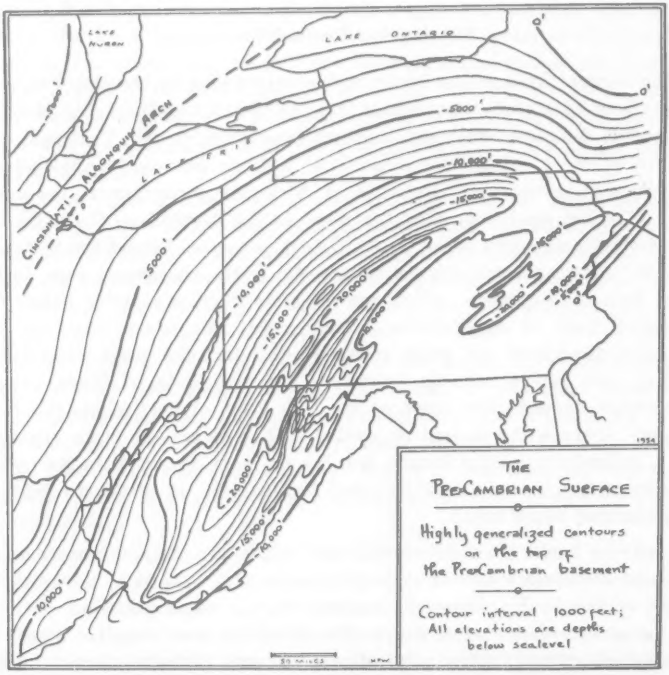

FIGURE 1

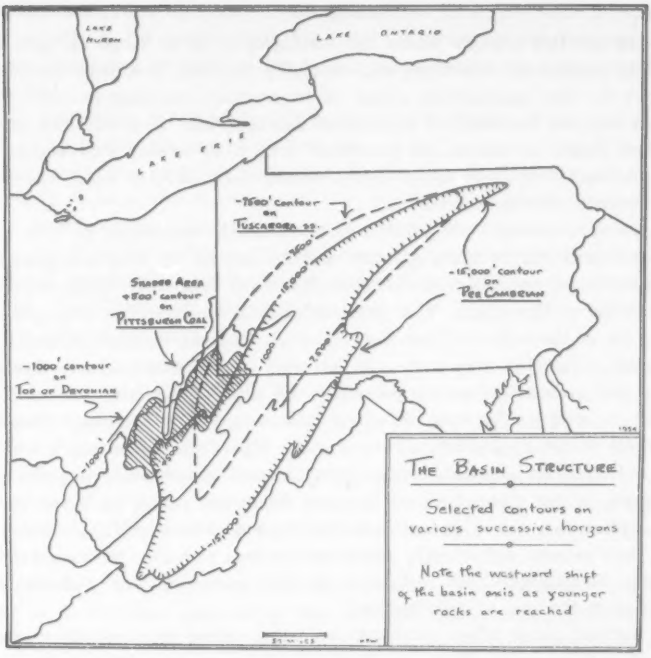
LAKE HURON
LAKE ONTARIO
LAKE ERIE
-7500' contour on TUSCARORA SS.
-15,000' contour on POCONO
SHADED AREA +700' contour on PITTSBURGH COAL
-1000' contour on TOP OF DEVONIAN
THE BASIN STRUCTURE
Selected contours on various successive horizons
Note the westward shift of the basin axis as younger rocks are reached

FIGURE 2

sag of the formation in question. It also means in the west half of the basin on the long upslope toward the Cincinnati arch that, except for early stratigraphic barriers to migration, or unless there were early-stage folds to prevent complete escape, there may have been adequate structure, ample time, and sufficient hydrostatic incentive for the complete loss of all volatiles long ago. This need not hold for the east half of the structural basin where secondary structures—*i.e.* the anticlines of late Paleozoic origin—have greater magnitude than the westward dip of the basin rim.

Deep as the basin is—more than 22,000 feet in its central portion—improved methods of modern drilling now bring practically all portions within reach of the drill and even, by use of rotary drilling, within sight of the geologist. The deepest well yet drilled in the area is less than 13,000 feet deep, but this record will doubtless soon be surpassed. A great deal of current interest follows the Hope Natural Gas Company's rotary well now drilling on the Burning Springs anticline, out of which an excellent 4-inch core of many critical formations is already being studied, some sections coming from depths of more than 7,500 feet below ground.

Stratigraphy

There are few regions where the stratigraphy of so large an area has been so generously described and carefully studied. The total thickness of rock in the Appalachian basin is tremendous, running to more than 40,000 feet as the total of individual maxima, and to nearly two thirds of that figure in actual or projected totals. It seems extravagant to recount many details of Appalachian stratigraphy save to emphasize one or two easily overlooked points.

It is not always remembered that internal variations in the rock column from north to south affected the folding which later crumpled the sediments and produced stresses at the north that were quite different from those at the south. The great thickness and relative homogeneity of the top of the column (Chemung, Catskill, Pocono, Pottsville) stiffened the rock column at the north and enabled great open folds to develop under the mountain-forming pressures of the Appalachian revolution. South of central Virginia, however, the thick post-Onondaga clastics pass out of the rock section, leaving the top of the latter much weaker and thinner. As a result, from this latitude southward, Appalachian structure is not characterized by open folds but rather by large thrust faults wherein great slices of disturbed rock slid forward (*i.e.* westward) until they became sufficiently stable to transmit pressure to more western blocks. Stratigraphy, as indicated in this example, had a controlling effect upon the type of deformation.

A second point often overlooked is the rather unusual shortage of unconformities in the bedrock. As one approaches the Cincinnati arch, there are numerous minor breaks or unconformities that appear in the rock column; yet for the basin as a whole, save the ubiquitous Pre-Cambrian break, there is no widespread or even extensive stratigraphic unconformity for the Appalachian geologist to contend with. Along the buried Adirondack-Tazewell axis, there are minor or localized breaks sufficient, for example, to remove all but five feet of the Silurian system at one exposure in northern Virginia; but these unconformities are quite localized, and in greatest areal extent only affect the early Ordovician. There is a broad unconformity below the middle Ordovician in Ohio east of the Cincinnati geanticline, with the Beekmantown dolomite being absent across a wide region of eastern Kentucky, eastern Ohio, and parts of western Pennsylvania. This unconformity had led to confusion in certain stratigraphic interpretations of wells drilled through the hiatus, in which there are sands of late Cambrian age that have been described as "St. Peter." Other unconformities occur below the Pennsylvanian System in southwestern New York State and below the Conestoga limestone in southeastern Pennsylvania, but the number and extent of these breaks are small.

Doubtless there are still other unconformities that are yet unknown. The Burning Springs anticline, for example, seems to be an ancient

structure that may be partly contemporaneous in origin with the Cincinnati anticline. Its deeper beds have not yet been drilled, but one might expect localized unconformities to occur on the flanks of so ancient a structure.

Another matter not to be forgotten is the contribution of early sediment spread eastward into the basin from the central interior. This is particularly important in parts of the Cambro-Ordovician section where the sand-speckled Appalachian dolomites of the late Cambrian and early Beekmantown grade westward into an essentially arenaceous series that cascaded from the Wisconsin dome. It may even be that some middle or lower Cambrian sandstones were washed eastward into the Appalachian Basin before the Cincinnati geanticline became a barrier to such lateral distribution of material.

Since the interest of the oil and gas geologist is heavily slanted toward sandy rocks, the western extension of the thick basal Cambrian sediments below the basin and the age relations of the thick Ocoee series of North Carolina still need solution and geographic definition, while the troublesome ages of the Talledega slates of Georgia and the Glenarm metamorphic series of eastern Pennsylvania, and the related matter of the Martic overthrust fault all bear upon the proper interpretation of the basin.

In passing, one might remark that for some of these latter problems, there is a nostalgic parallel in the story of Ebenezer Emmons and his "Taconic Problem." As nearly everyone remembers, Emmons believed that he had discovered a new series of rocks older than the Cambrian but yet not part of the crystalline complex. When the facts were finally focussed, it was clear that he was dealing in part with rocks that were actually older than the Potsdam sandstone—then regarded as the bottom of the Cambrian—and in part with rocks that were altered equivalents of known formations. Perhaps some of these present stratigraphic problems will as easily fall into proper place as did the controversial "Taconic System" of Emmons. In the meantime, one can hope that no one will die as Emmons did—embittered at the nonacceptance of his views.

The shore zones of Appalachian formations take on special meaning in relation to the occurrence of oil and gas, for most Appalachian production is controlled more by facies change than by anticlinal structure. Such things as updip permeability wedge-outs, gradation of sand into shale, sandbars and beaches, and border patches of no-sand deposition, all function heavily in the present search for oil and gas in this basin. Fracture porosity, leaching, recrystallization, and the wedging of enclosing impermeable strata likewise represent stratigraphic matters that profoundly affect producing pools in the region. Some of these factors were contemporaneous with deposition; others were induced by events immediately following deposition; a few have had more recent

history. Their total contribution to the present location of oil and gas, and to the search for future supplies is an important matter.

In this connection one must remark that a great part of the successful Appalachian production shows small genetic relation to late surface-visible folding. Most exploratory wells have been drilled in accordance with the anticlinal theory, if for no other reason than to reach older beds at shallower depths. This principle could be both wise and unsuccessful, or it could be unsuccessful because it is unwise. At present it is largely unremunerative, and it is urgently advised that serious deep future exploration be patterned on the hypothesis that sedimentary and paleogeographic conditions at the time of deposition have had greater effect on the location of present reservoirs than postdepositional structures related to orogenic deformation.

Finally, one should note the increasing number of dikes and stocks of igneous rocks that are being discovered in the Appalachian country. These consist mainly of small peridotite dikes that occur singly or in groups from New York at the northeast through Pennsylvania and the two Virginias into eastern Kentucky. More than 100 are now known, and their small size and obscure exposure suggest that only a low proportion has yet been described. Their significance is mainly historical, of course, yet their distribution may signify some critical function of the crystalline basement that had other importance as well. The interesting Beemerville nepheline-syenite of northwestern New Jersey is a case in point. Of late Ordovician or pre-Silurian age, it strengthens the theory of a considerable phase of late Ordovician orogeny that extended well into the basin itself. There are those, indeed, who regard the Taconic disturbance as the major Appalachian orogeny, with all later Paleozoic movements being merely the after-effects of the pre-Silurian orogeny.

Folding and Deformation

The final construction of the Appalachian structural basin near the end of the Paleozoic Era closes one of the most familiar of all orogenic series, and the spectacular Appalachian folds are the American prototype for a sequence of deposition, deformation, uplift, and erosion that has been thoroughly studied by several generations of students. It would be absurd to compress in this short paper any analysis of the mechanics or history of this folding; yet despite long study and broad familiarity, many critical questions remain unanswered.

Although it is believed that overloading by Paleozoic sediments was responsible for the diastrophic events that closed the era, the exact interrelations are by no means clear. It is probably sufficient to observe, although the overloading of any depression appears to have orogenic consequences, that neither the exact volume of sediment nor the length of time involved seem yet to provide any quantitative measure of the

character, intensity, or time of inception of the succeeding deformation.

It is clear that the greatest effects of all Appalachian orogenies involved sediments deposited in the eastern half of the old geosyncline—if one is still entitled to speak of geosynclines. The tremendous thickness of sedimentary rocks is a reliable indication that the trough was progressively sinking during most of the Paleozoic era, while the inferred height of its eastern border during Pottsville time attests that the latter had regained whatever altitude had been lost by Paleozoic erosion. It is surprising that such substantial settling was accomplished without essential deformation of the eastern and central basinal areas during early and mid-Paleozoic times. It is true that there was a vast and intense orogenic revolution at the end of the Ordovician, and at least one other major disturbance near the close of the Devonian; yet the diastrophic effects of these early and mid-Paleozoic uplifts materially failed to disturb intimately or involve the thickest column of sediments already deposited, and their effects are localized in the area under consideration mainly to the east flank of the basin proper. Unless the intermittent and small upward movements of the Cincinnati anticline represent some compensation for the subsidence of the old basin, then the western flank of the trough and the area adjacent thereto have long remained neutral and undisturbed.

The main structural basin is obviously foreshortened in a general northwesterly direction, and all data attest that the deforming forces acted in that direction. Overthrust fault-planes dip southeastward at more or less gentle angles; folds are mostly overturned toward the west or show steeper western slopes. The dwindling of folded structures toward the west and the absence of any metamorphism in that region both affirm diminishing pressure distant from its source. This general western compression is a function of all three of the major Appalachian orogenies—Taconic, Acadian, and Appalachian proper. How it was brought about, whether by plastic or elastic crustal deformation, by isostatic readjustment, by tectogenic infolding, or by whatever cause, we cannot digress to contemplate in this short summary. The effect, however, was that of laterally directed pressure producing folds, faults, and crustal shortening on a grand scale.

The distance of 300 miles that separates the Cincinnati geanticline and the main Appalachian folds of Pennsylvania was shortened to 50 miles in central Tennessee and northern Alabama. The difference between these figures is a fair measure of the amount to which the Appalachian basin has been crowded westward. The telescoping of its tectonic elements through folds and faults extensively foreshortened the former continental border. To restore to its pre-Permian longitude any present point in the folded Appalachian basin would involve its eastward shifting by many tens of miles. It is thus imperative, in constructing paleogeographic and historical maps of the region, to take these matters into

consideration by use of palinspastic maps of the type described by Kay.

One must also surmise that the effects of the Appalachian deforming force were torsional rather than completely normal to the strike of the present orogenic belt. The prevalence of overthrusts in the southwest portion of the Appalachians is already partly attributed to the weakness of the stratigraphic column in that latitude, but it also suggests a variation in the strength of the deforming force. It seems likely that the orogenic pressure of the Appalachian revolution did not act uniformly along its entire front, but increased somewhat in intensity from north to south. Doubtless there was a migrating focus of maximum pressure which seems to be clearly exhibited in the torsional movements of the Cumberland fault block of southwestern Virginia and eastern Kentucky. Such a southwestward shifting of the major deforming force, together with a westward migration of the zone of active deformation across the folds, could be partly responsible for the present divergence of Appalachian folding from the course of the original Appalachian geosyncline.

But the direction of pressure cannot everywhere have been westward. The small or low amplitude folds of southern New York and northern Pennsylvania all strike essentially west-east to cross the Adirondack axis nearly at right angles. Furthermore, it is plain that while late Appalachian folding reached southern and eastern New England, it seems not to have affected Vermont or New Hampshire. This strongly suggests that the main strike of Appalachian folding should extend roughly eastward from the Susquehanna region of Pennsylvania, rather than northeastward up the Hudson as is generally believed. Such a course requires a major component of northward pressure rather than wholly westward or northwestward pressure as is commonly assumed.

It now seems clear that the true belt of familiar Appalachian folds still needs to be accurately traced across eastern Pennsylvania and southeastern New York, and the results of such studies may prove surprising.

West of the Valley and Ridge region where the late Paleozoic deforming pressure had largely spent itself, the foreland tract of the main basin was mildly wrinkled with small widely spaced folds that decrease in numbers and intensity westward until along the Cincinnati geanticline all effects of the Appalachian revolution completely disappear. As will be noted below, these secondary folds, it is believed, progressively cross in a southwestward direction the earlier troughs and basins produced by Taconic warping.

One must attribute most of the present grain of the Appalachian country to the late Paleozoic deformation. The sinuous course of the folded ranges, the strong southwestward strike of the major folds, and the present pattern of the bedrock are all features of Appalachian folding. Save in the northeastern portion of the Appalachian highlands,

almost all surface trace of the older Taconic deformation has been obliterated.

It has been impossible in preparing these few paragraphs about the younger deformation to omit reference to the older and less well-known Acadian and Taconic orogenies that distorted large portions of the New England-Maritime provinces and whose echoes or shadows affected the Appalachian basin itself. It is not clear how far south or west Acadian folding intimately disturbed the Appalachian sediments. I suggest, however, that the familiar little folds of the Kingston-Catskill-Helderberg region are not true Appalachian folds but belong to a different, probably earlier generation. I suggest they reflect pressure translated westward across the Hudson Valley by the resistant Cambro-Ordovician limestone, and I suspect them to be of Acadian (late Devonian) age.

There are some matters connected with the great Lackawanna syncline (the northern anthracite basin) that need clarification and intensive field study, for it looks as if there are two quite separate post-Pottsville fold patterns in that basin. I suggest even in the well-studied and geologically famous suburbs of the New York metropolitan area that one can find innumerable structural problems in the northeastern Appalachian basin that have barely been stated as yet.

In the deep Appalachian basin, if there are no major traces of Acadian movement, there are numerous indications of minor isostatic Devonian adjustments. The Oriskany sandstone thins perceptably on the crest of the Warfield-Chestnut Ridge anticline as well as on certain other major structures. This must indicate active Devonian movement, and it becomes critical for the oil and gas geologist to ferret out such indications to ascertain whether pre-Mississippian folding foreshadowed later Appalachian movements and predetermined their fold patterns, or whether Devonian deformation followed a pattern of its own that is not mirrored by surface-visible folding.

The character and geography of the Taconic deformation of pre-Silurian age badly needs to be worked out for the main structural basin. The true nature of the Adirondack-Tazewell arch has only recently been appreciated as something of a miniature Cincinnati anticline that affected both structure and stratigraphy, and the possibility that there are other Taconic foreland folds now buried or concealed should be carefully explored. Finally the influence of the ancient Pre-Cambrian crystalline basement on later deformation and stratigraphy requires study that has been too long postponed.

Wide and deep, physically, as the Appalachian basin is, the problems concerning its deformation and structure are broader and more fundamental than many of its better-known properties, and one must realize that our full knowledge of the Appalachian country is extremely hazy at best and amateurish at worst.

Appalachian Oil and Gas

As the Appalachian region was the first oil and gas basin to be developed in this country and has been continuously productive for nearly 100 years, its rocks have been pierced by more wells than almost any analogous region. A great deal of information is now known as to its subsurface structure and the trends of productive horizons. It was here that I. C. White first developed the "anticlinal theory" of oil accumulation—a principle still followed intentionally or unwittingly by most operators in the region. Nevertheless, despite its apparent successes, the "theory" is only locally valid, and it has been said with some truth that its promotion may have retarded rather than advanced the development of the oil industry as a whole.

Study of various Appalachian-producing horizons indicates that the strike of the actual pools or fields does not conform to the strike of the structural basin, but rather holds more of a north-south direction, parallel to the axis of the Cincinnati-Algonquin arch (see FIGURE 3). This concordance clearly attests that at least western Appalachian oil and gas production is not controlled by the trend of the deep trough of the

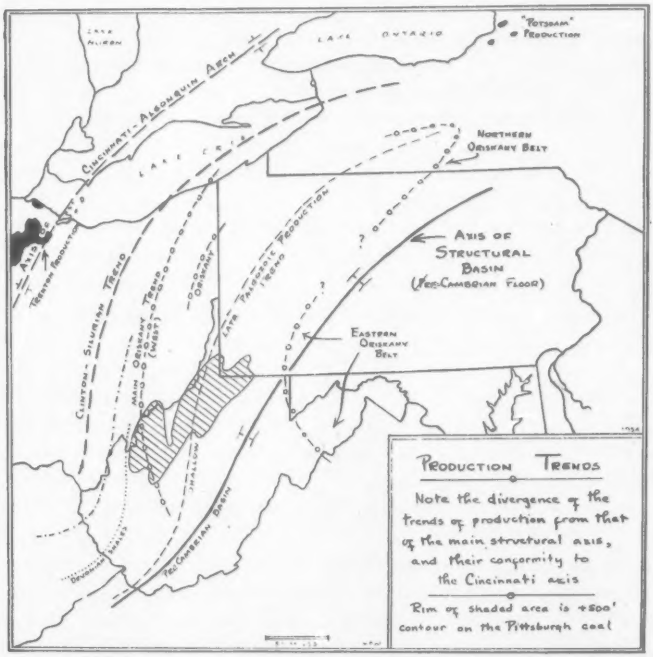
LAKE ONTARIO
"POTSDAM" & PRODUCTION
CINCINNATI-ALGONQUIN ARCH
LAKE ERIE
NORTHERN ORISKANY BELT
AXIS OF STRUCTURAL BASIN (PRE-CAMBRIAN FLOOR)
EASTERN ORISKANY BELT
CLINTON-SILURIAN TREND
PRODUCTION TRENDS
Note the divergence of the trends of production from that of the main structural axis, and their conformity to the Cincinnati axis
Rim of shaded area is +500' contour on the Pittsburgh coal

FIGURE 3

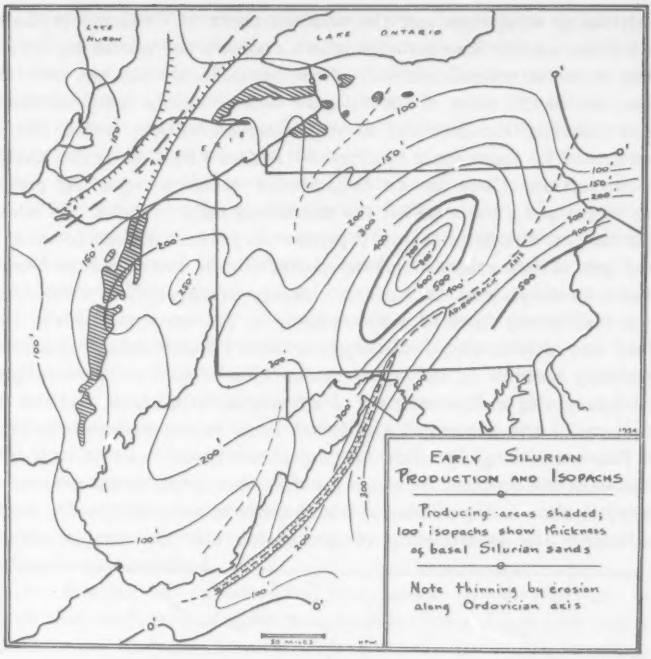

FIGURE 4

structure nor by the pattern of the major folds, but rather follows the marginal zone in which producing sands lap westward upon the flank of the Cincinnati anticline.

Thus the areas of Silurian production maintain a constant location about 60 to 70 miles east and south of the crest of the geanticline and follow the westward shoaling of the several beds from which the production comes. Within such Silurian oil and gas fields of north-central Ohio and western New York, commercial production is concentrated by the updip convergence and pinch-out of various sands. Wedges and lenses were created under environmental and depth conditions controlled by shoaling of Silurian seas against the rising arch; and much as one field differs from another, most Silurian production must be attributed to original stratigraphic entrapment environmentally effected by the Cincinnati arch, rather than to structural entrapment genetically related to Appalachian folding (see FIGURE 4). In this belt of Silurian production, the so-called anticlinal theory is of small consequence and its principle could not have been developed from observation or practice in these fields.

Much the same relation exists between the western feather-edge of

the Oriskany sandstone and the western areas of Oriskany production. Again it is not the basinal axis which controls the producing area, nor do the major structural features of the basin locate the several fields. Rather, as in the case of the Silurian sands, it is a marginal environmental relation that provided stratigraphic entrapment which has been concentrated by subsequent folding (see FIGURE 5). Concerning Oriskany gas production, there is an unanswered problem regarding porosity; for it is not yet clear whether the sandstone only contains gas where it is porous, or whether it is very porous only where it has or once contained gas. The northern Oriskany production in southern New York and northern Pennsylvania is likewise largely stratigraphic in distribution but is differently located with respect to the structure of the basin.

The late Devonian, Mississippian, and Pennsylvanian production—essentially shallow in character—seems less related to the stratigraphy of the containing sediments than to superficial structures, and the latter are primarily the series of small and large folds produced during the late Paleozoic orogeny. Here the anticlinal theory was first projected and is widely accepted, although the fact that many areas produce from channel or shoe-string sands, or from bars or other stratigraphic wedges, complicates the relationship of production and the simple anticline.

FIGURE 5

Some of these pools even produce from synclines. The strike of this large belt of shallow production, as in the case of Oriskany and Silurian belts, does not parallel the main axis of the basin, but rather follows a trend that is also aligned with the Cincinnati arch. Production within the belt rises from late Devonian sands at the north to Mississippian and Pennsylvanian sands toward the south.

It may be noted on the west side of the basin that oil is more prevalent in the western pools of the shallow belt, while gas production is heaviest on the eastern side. To account for this geographic distribution, David White developed the "fixed carbon" principle, linking the nature of the contained hydrocarbons to the amount of metamorphism as revealed by the fixed-carbon content of nearby coals. Enough exceptions to the principle—such as the occurrence of oil in highly folded and faulted beds of southwest Virginia—have now turned up to suspect that something is wrong with the theory. Indeed, much present exploratory drilling for the older and deeper rocks of eastern West Virginia and Pennsylvania is now starting in or near beds that completely fail the carbon-ratio test.

As can be expected from the trend-lines of earlier and later productive horizons, the fields that produce from the mid-Silurian, early Devonian, mid-Devonian, and late Devonian properly align themselves between the producing belts described.

In the absence of any conclusive evidence to the contrary, it is believed that each producing horizon supplies oil and gas that may have migrated some distance laterally but probably have not wandered at random across bedding planes from rocks of much younger or older age. In short, the material is thought to be largely indigenous or to have been derived from nearby sources. Too, it is thought to have been generated and initially implaced not long after rock consolidation rather than subsequently introduced long afterwards. If these beliefs are correct, then production from Silurian horizons could have been found during the Devonian had someone been looking for petroleum. In the same fashion, Oriskany oil and gas were probably in existence while Berea sands were being spread in eastern Ohio. It is no contradiction of logic to wonder if Ordovician or Cambrian production was potentially available during the Silurian, and if so, why it too might not still be available if sought at the proper place.

Since the younger rocks of this region are rather thoroughly catalogued as to their oil and gas possibilities, attention has recently turned to the deeper potential and older horizons of the basin itself. There are several plausible targets. The early Cambrian sandstones are not believed to show metamorphic symptoms far west of their present exposures, and their outer or western margins may still be suitable for the preservation of any volatile hydrocarbons once contained in them or received during later migration of oil or gas. The Trenton limestone which produces much oil in the Lima-Indiana field underlies most of the Appalachian

basin, and if conditions of recrystallization or fracture porosity can be located–say along some ancient arch or structural line–then this limestone could hold the key to deeper production in the Appalachian country. The western feather-edge of the Oswego-Bald Eagle sandstone is not yet thoroughly defined nor at all drilled; it could provide the same kind of traps that mark the productive basal Silurian in eastern Ohio. Much of the Oriskany sandstone is still untested, and the possibility that there is a separate Oriskany basin east of the Adirondack arch should be seriously considered. Finally, to the search for production near the eastern metropolitan region must be added the urgent need for underground storage facilities, and it is not impossible that certain unlikely producing horizons–even including the Triassic or crystalline Pre-Cambrian rocks–may warrant study as potential storage reservoirs.

Oldest and long greatest of American producing regions, the Appalachian basin is still an important factor in oil and gas production of this nation. It is difficult to believe that the ingenuity of the geologist will fail to add substantial gains to the known reserves still undepleted. Perhaps the most urgent need is to interest geologists in this problem and to stimulate further study of the region as a whole. Other producing areas are far ahead of the Appalachian country in recognizing the importance of applying intensive and skilled research to the solution of mutual problems, and the region is so broad and its many facets so casually understood that the pure scientist is apt to turn his attention to some minor detail or specific matter.

I suggest to you, as I have recently done to professional geologists of many companies operating in this basin that the Appalachian country still represents a region where there is great need for general and cooperative study of many unsolved problems, and I recommend that you give more consideration to some of the matters that require your own special talents and experience. The oil and gas geologists look to the pure scientist to tackle some of these problems, while the scientist assumes that the petroleum geologist will work out his own answers. There is ample room for both to work, and both should share the job.

This paper is one of three which I have lately presented in an effort to redirect geologic attention to a fascinating and critical region that urgently needs restudy and new perspectives. Who better than the geologists of this distinguished Academy could be asked to place the further study of the Appalachian structural basin high on their agenda of unfinished business?

SECTION OF BIOLOGY

STUDIES ON LIVING HUMAN OVA*

By Landrum B. Shettles**

Department of Obstetrics and Gynecology, College of Physicians and Surgeons, Columbia University, and the Sloane Hospital For Women, New York, N. Y.

Phase-contrast microscopy was used to gain a better cytological understanding of living human follicular and tubal ova before and after their exposure to spermatozoa *in vitro*.

Follicular ova were aspirated by means of a syringe and needle from ovaries *in situ* or immediately upon their removal. The follicles ranged in size from 3 to 16 mm. in diameter.

Tubal ova were washed from Fallopian tubes immediately upon their removal from patients whose ovaries contained a recently ruptured follicle; a pipette and homologous blood serum were used.

The ovum upon removal or passage from the follicle is normally surrounded by the corona radiata and some cumulus cells. After a drop of fresh normal semen is added to the follicular fluid or blood serum and the mixture kept at 37° C. for 24 hours, there is partial dissolution of the corona and cumulus cells. When small pieces of homologous tubal mucosa are also added, denudation is complete or nearly so by the end of three hours. No dispersion of the cumulus cells occurred in controls. Hyaluronidase, added to the follicular fluid instead of the semen and tubal mucosa, failed to effect dissolution of the corona or cumulus cells. These cells around the ova treated with hyaluronidase, and in the controls after 24 hours of incubation, remained firmly adherent to the zona pellucida upon agitation. Aspiration and expression of these ova several times through a very fine pipette failed to show any evidence of dissolution of these cells. By the end of three hours, the same treatment of the eggs in the presence of fresh normal semen and tubal mucosa usually resulted in ova surrounded by only the zona pellucida. The diameter of the living ovum, to the outer surface of the zona pellucida, was more than 90 microns in the majority of the specimens, the largest measuring 150 microns. Over 300 ova were studied.

Within the tube, complete denudation of the corona radiata and cumulus cells occurs. The exact time required for this denudation is not known. However, one such ovum was obtained approximately 24 hours after ovulation, as judged by the time in the menstrual cycle. The first polar body was seen clearly.

It has been observed that homogenized human tubal mucosa is fibrinolytic and contains fibrinolysokinase, whereas the follicular fluid usually

*This paper, illustrated with photographs and motion pictures, was the first of two papers presented at the meeting of the Section on November 8, 1954.

**John and Mary B. Markle Foundation Scholar in Medical Science.

contains large amounts of antifibrinolysin and, also, high concentrations of profibrinolysin; the follicular fluid also can be made fibrinolytic through the action of the tubal mucosa. Therefore, it is believed that, in the denudation of the ovum, its penetration and fertilization by the spermatozoon, the fibrinolytic enzymatic action of the tubal mucosa plays a very important part.

Soon after the semen is added to the follicular fluid, the spermatozoa surround the cumulus and corona radiata. After denudation, the spermatozoa cover the surface of the zona completely. In no case was there any evidence of a trophic influence of the egg on the spermatozoa. Spermatozoa may swim in the closest proximity to the egg and yet pass by without any apparent attraction. However, having once accidentally touched the ovum, the vast majority remain in contact with it. At no time was there any falling away of the spermatozoa from the zona pellucida or suggestion of a fertilization membrane forming. With hundreds of spermatozoan heads rotating against the zona pellucida at right angles or nearly so, tails moving vigorously, the ovum is very often rotated in the clockwise direction. It is often moved about within the medium and simultaneously rotated. The maximum rate of rotation was 360° in 15 seconds, which continued for as long as 18 hours. This phenomenon was observed repeatedly, while all other cellular particles within the medium remained stationary. The spermatozoa rotated on their longitudinal axes, with their heads against the zona pellucida. The effective stroke of the tails probably produced the rotary movement of the ovum.

Since spermatozoa come in contact with the egg by chance, and since more than one spermatozoon may pass through the zona pellucida and perivitelline space into the ooplasm, it is believed that the one spermatozoon which enters finally into the formation of the male pronucleus may well be determined by chance approximation of the male to the female pronucleus.

The nucleus is seen often very clearly, round and eccentric in the ooplasm. There is a single nucleolus within the nucleus, homogeneous in consistency, almost round, and also eccentrically located. When dissected free, the nucleolus remains intact. The nuclear membrane is uniform in thickness and perfectly round in outline. Chromosomes in the tetrad stage have been seen.

During part of its existence, the human oocyte contains the yolk-nucleus, a small spheroidal body within a crescent shaped pallial layer. The pallial layer is found only in the earlier stages of the oocytes and does not long persist as such. It has been believed to consist largely of chondriosomes, as well as Golgi bodies and, also, of nucleolar material. The yolk-nucleus-pallial layer complex accordingly shows a close analogy to the idiosome-complex of the spermatocytes, and the two may be regarded as homologous formations.

Globules have been noted within the perivitelline space in many of the

eggs. They are homogeneous, translucent, and vary in size, having the appearance of fat globules. Granules of different size, shape and density may be present in the perivitelline space. This space and its contents usually extend to, and lie in, such close proximity with the inner surface of the zona pellucida that no space is detectable between the two. The zona pellucida has been stretched so as to be pulled away from the perivitelline area in part of its circumference. There appears to be a very definite limiting boundary or membrane surrounding the perivitelline contents. This membrane is occasionally seen in ova without dissection; normally, however, it is a potential space.

The vitelline or cytoplasmic membrane of the ovum is represented by the limiting boundary of the cytoplasm. The constituents of the cytoplasm are distributed evenly throughout. Of all the ova studied, only one showed a distinct vitelline membrane.

Movement of the vitellus within the zona pellucida is often observed in the denuded normal ovum upon slight agitation. The larger ova are eccentric within the zona pellucida. As a result, when the eggs are rolled about, a definite polarity is demonstrated; *i.e.* they lie finally with the narrowest portion of the perivitelline space between the ovum and the surface of the container. This tendency may determine the position in which the segmented egg embeds in the uterine mucosa.

The zona pellucida is found invariably to contain interlacing fibrils, on which are located dark, fine granules. This network of fibrils extends throughout the zona pellucida. Under oil immersion, pressure upon the cover glass results in a decrease in the interfibrillar areas and an attenuation in the diameter of the fibrils. Release of the pressure shows these changes to be reversible. Under lower magnification, pressure upon the cover glass results in an increase in the circumference and decrease in the thickness of the zona pellucida. Release of the pressure shows these changes to be also reversible. The elasticity of the zona pellucida may help in maintaining the integrity of the cleaving and early expanding cellular mass.

One ovum after 10 hours of incubation at 37° C. with spermatozoa contained the first polar body and spermatozoa in the zona pellucida. The first and second polar bodies have been observed in other ova after 24 hours of incubation. Spermatozoa were also present in the zona pellucida and perivitelline space of these cells.

By the end of 24 hours of incubation with spermatozoa, one ovum contained three polar bodies; the first polar body had divided. The zona pellucida was opened, and the ovum, as well as the three discrete polar bodies, were removed by means of glass dissecting needles; the freed egg was then washed through 10 transfers in physiological salt solution by means of a micro-pipette, after which examination revealed an entire spermatozoon within the ooplasm. The first polar body may undergo division before the egg is fertilized or before the second polar body is released.

Since the whole spermatozoon has been observed in the human ovum, more credit is probably due the male in regard to his cytoplasmic contribution to the offspring than has heretofore been rendered him.

After 30 hours of exposure to spermatozoa *in vitro* and blood serum to which tubal mucosa was added, a 2-cell stage was observed. After 50 hours a 4-cell stage of an ovum which contained three polar bodies was obtained and, after approximately 60 hours of exposure, an 8-cell stage appeared.

SECTION OF BIOLOGY

MICRODISSECTION STUDIES ON HUMAN OVARIAN EGGS*

By William R. Duryee

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Microsurgical methods applied to single cells are becoming ever more useful. While the structure of mammalian eggs appears deceptively simple, the physiology of egg growth, maturation, and development is overwhelmingly complex. Human ova are of unusual interest in the mammalian category, if for no other reason than Alexander Pope's unanswerable dictum that "the proper study of mankind is man." Since improved techniques of micromanipulation were available, largely through the pioneer work of Robert Chambers, it seems worthwhile to describe in this paper some observations on fresh, unfixed, and presumably living human ovarian eggs conducted by micrurgical methods. The author is deeply indebted to Doctor Corner and to the late Doctor Streeter of the Carnegie Institution's Department of Embryology, to Doctors TeLinde and Vosburgh of the Johns Hopkins University Hospital, and to Doctor H. L. Darner of Washington, D.C., for fresh ovarian material obtained through routine hospital operations. A temporary laboratory was established immediately outside the operating rooms. This laboratory was found to be indispensable because, in cases where freshly excised tissue was transferred from one building to another with resultant lengthy exposures to Ringer-Locke solution and to temperature changes, significant degenerative changes were observed.

A technique was devised to incise a portion of the ovary in a deep Petri dish of warm Ringer-Locke solution. If the sharp cut passed through a follicle, the two halves were examined under a binocular microscope for the cumulus. When found, this mass was transferred by means of a fine pipette to a large coverslip and mounted as a hanging drop on the moist chamber. Excess cumulus cells were carefully removed with microneedles, so that the ovum with its corona cells could be observed in excellent detail and further dissection carried out. In all, about 27 ova were observed, with dissection studies on 15. In the other cases, crushing experiments or effects of reagents were attempted. It was found that the age of the donor was important, the best follicles being in the 21 to 35-year group. Ovarian tissue from older patients were apt to be too fibrous and suitable follicles extremely difficult to locate. The chief findings of this study center on (1) attachment of the corona cells to the ovum; (2) the jellylike consistency of the zona pellucida; and (3) the structure of the egg nuclei, of which four were dissected free.

*This paper was the second of two papers presented at the meeting of the Section on November 8, 1954.

Attention is directed to the excellent paper of Allen, Pratt, Newell, and Bland (1930), which stands out as the best work summarizing previous knowledge of follicular ova. In contrast, the more recent presentation of Dalcq (1952) barely mentions human ovarian eggs. While Dalcq has developed some excellent techniques, his one photograph of a stained section of an immature ovum indicated the erroneous egg membrane-zona relationships so common in our standard histology textbooks. One partial exception, kindly brought to my attention by Doctor Hartmann, is the observation of Moricard (1936) on the *vacuome radié* of the fixed rabbit egg.

Observations

In freshly isolated ova from medium to large follicles (9 to 15 mm.) mounted in warm Ringer-Locke solution (pH 8.0) corona radiata cells were observed tightly packed in three or four layers about the egg. Ovum-follicle cell relationships at first were difficult to establish even with strong illumination and microneedle compression. The appearance of the ovum and zona pellucida suggested cell-wall contracture and led to development of a new mounting medium, based on the author's calcium-free Nuclear Medium for isolated amphibian egg-cell nuclei. A satisfactory solution was NaCl 0.85 gm., KCl 0.025 gm., double distilled H_2O 100 ml. For short exposure periods up to 20 minutes buffering with $NaHCO_3$ to pH 8.0 was adequate. It is quite possible that a more complex sterile medium would have been better, but facilities for preparation were not available. The Ca-free saline did not condense and thicken zona pellucida jelly, nor did it cause the radiating tubules of the corona cells to contract and rupture. Consequently slow stretching of corona cells away from an ovum, using two needles cemented with plastic resin as a holder and a third needle for traction, permitted clear observations and photography of coronal canals (see diagram in FIGURE 1: *C.C.*). An unusual ovum structure at the vitelline surface then became apparent. Covering the ovum surface as outpouchings of the vitelline membrane (*V.*) were very numerous thinwalled projections, which may be designated *vitelline cones* (*V.C.*). Each, in turn, connected with the previously described *coronal canal*, occasionally referred to in the literature as a "process." The structure of the cones was extremely delicate. On many ova they would retract spontaneously into the vitelline membrane, thus rupturing their connections with the coronal canals. At the same time, zona pellucida jelly (*Z.J.*) was seen to become more dense and slightly opaque. When ordinary Ringer-Locke solution was substituted, coronal canals became more brittle and were more easily ruptured by stretching. Vitelline cones always retracted and disappeared on addition of standard fixatives.

In younger, less mature ova, vitelline cones may be examined in various stages of development. FIGURE 2 illustrates the condition

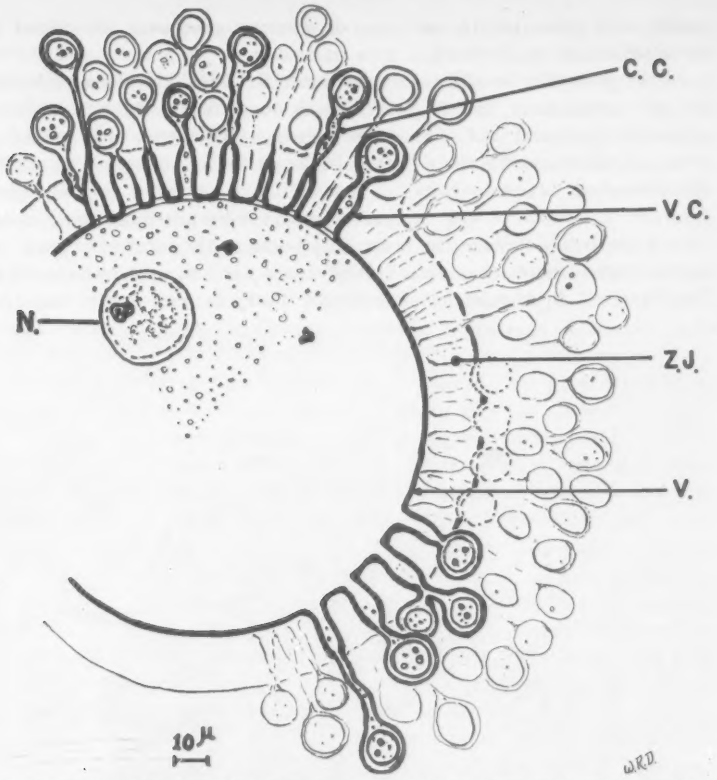

FIGURE 1. Diagrammatic representation of human egg with corona radiata cells. Drawn from unfixed specimen isolated from a 14-mm. ovarian follicle. In upper and lower thirds of diagram, the cones have been heavily outlined for clarity. In middle third, the light lines more accurately represent the fresh condition. C.C., coronal canal; V.C., vitelline cone; Z.J., zona jelly (in zona pellucida); V., vitelline membrane; N., nucleus.

before the zona pellucida has fully formed. The view was obtained by raising the egg up against the cover of the moist chamber so that the cones and coronal canals were slightly flattened. It will be noted that the nucleus was displaced against the vitelline membrane.

A proof of physiological continuity of the coronal canals with egg protoplasm was given by the experiment whereby ambient Ringer-Locke was diluted with distilled water to give a hypotonic medium approximately two-thirds full strength. The corona cells swelled in progressive sequence from outer layers toward the inner. Concurrently droplets of fluid were observed accumulating beneath the vitelline membrane. Hence the corona tubulae were functional and acting as canals. Cytoplasmic particles were also observed mobile in the canals and cones, and hence

could well give rise to the type of granular processes described in fixed rabbit ova by Moricard.

In the preovulation mature stages described here, the zona pellucida is not membranous, but is jellylike in consistency. Superficially, it appeared to cement and support the corona cells, which also exhibit a most interesting network of intercellular bridges. As far as could be determined it is *not* a capsule, nor is it fibrous as it appears after fixation. Exovates of egg cytoplasm obtained by micropunctures could flow quite freely through the zona jelly between the vitelline cones. At these stages, there is no perivitelline space such as appears later after fertilization. It should be remembered that, in very early immature

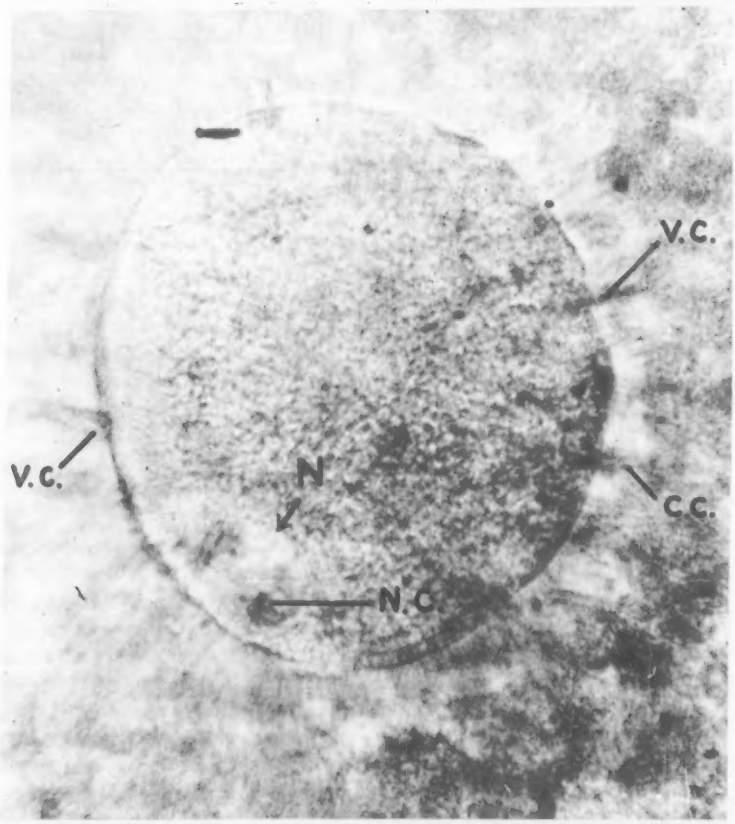

FIGURE 2. Freshly isolated human ovarian egg. Isolated from a 9-mm. ovarian follicle, mounted in Ringer-Locke solution, and slightly compressed by microneedle against coverslip. V.C., vitelline cones; C.C., coronal canal; N., area of the egg nucleus; N.C., nucleolus. × 940.

follicles, the young corona cells or inner theca layer start as a cuboidal epithelium, changing to columnar with age. At first these cells are closely applied to the vitelline membrane. This study supports and confirms the conclusions of Allen, Pratt, Newell, and Bland that, as mitoses of corona cells continue, these structures, analogous to skin cells, are pushed farther and farther from their basement membrane. A sure sign of a normal isolated ovum is the presence of mitosis in the inner layers of the corona.

Fresh human egg cytoplasm is coarsely granular with particles 1μ or less. No yolk platelets as such were observed. This cytoplasm, like the protoplasm of transparent immature amphibian eggs, is a viscous liquid capable of flowing through apertures. Inclusions of various sorts have been described by many authors. In this series, they ranged from one to six dense bodies approximately two to five μ in diameter to smaller round or oval droplet types in clusters near the nucleus.

The egg nucleus or germinal vesicle was isolated in only four cases and then with considerable difficulty. Unfortunately the new Bush-Duryee-Hastings electric micromanipulator was not available at the time our experiments were performed. With joy-stick operation and hand-needle ratio control it would have been considerably easier to open the ovum and drag the nucleus through the aperture. A microaspirating technique would likewise be desirable to free the nucleus immediately from its own cytoplasm—a step, which in amphibia, is necessary to prevent nuclear flocculation. No unusual features of the human germinal vesicle were noted. The large nucleolus (4 to 5μ) appeared as a typical colloidal coacervate mass attached in a "chromogel" near the centrally located expanded chromosomes. Like nucleoli of other cells, it is a plastic deformable gel with contained droplets. The nuclear membrane had properties similar to those described for salamanders and frogs (Duryee, 1950), which were shown to be sheetlike mosaic organoids with lipoprotein characteristics.

In more mature ova (diameter 135 to 145μ) from larger follicles the nucleus (diameter 37 to 49μ) was clearly displaced from the original central position. This displacement may well indicate an animal-vegetal axis of polarity as suggested by Dalcq. There was no indication, however, of a secondary lateral gradient of cytoplasmic density. It seems quite possible that Dalcq's observations of secondary gradients in other mammalian eggs is due to oriented penetration paths of his fixatives.*

No summary of nuclear-cytoplasmic relationships would be complete without reference to relative volumes. From photographs of these human

*Readers may be interested to re-examine critically serial sections of mouse and cat ovaries in their own histological collections and to note crescentic precipitations of stained granules against both vitelline and nuclear membranes. If an ovum is followed through a series of thin sections, the aggregations are usually *away* from the surface of the block of tissue as dropped into the fixative. These effects are due to differential rates of fixative component migration through the protoplasm and may be verified in isolated cell components subjected to unilateral fixation exposures.

ova it was estimated that between 3000 and 4000 corona cells are attached to each fully mature egg. If nuclear volume is calculated in relation to that of egg cytoplasm, we obtain a nuclear-cytoplasmic ratio of approximately 1:40 or, as expressed numerically, 0.025. However, if the volumes of unfixed corona cell nuclei are added to that of the germinal vesicle and compared to the overall volume of the cytoplasmic syncytium, we obtain a value of about 1.2, which is 50 times greater. From this value we may conclude that the nucleus, even of a human egg, must be substantially supplemented in building the cytoplasm for a new human being.

Summary

(1) Twenty-seven immature human ovocytes (from 97 to 145 μ diameters) were isolated from transsected follicles in physiological media.

(2) A low-calcium medium was found preferable to Ringer-Locke solution for microdissection of the eggs and surrounding cells.

(3) The ovum is nourished by multiplying corona radiata cells which attach to outpocketings or cones of the vitelline membrane. The canals of the corona are functional tubules capable of transmitting fluids and cytoplasmic particles to the egg. Vitelline cones are easily ruptured and disappear on fixation.

(4) The zona pellucida consists of a viscous jelly supporting the corona cells, and is not at this stage membranous in structure.

(5) When isolated, the nucleus of the human ovum presents the typical vertebrate egg structure: a sheetlike nuclear membrane, colloidal coacervate nucleolus, and a central chromogel for support of the chromosomes.

(6) The conclusion is drawn that approximately 3000 corona cell nuclei are necessary to supplement the synthetic function of the egg nucleus.

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SECTION OF PHYSICS AND CHEMISTRY

FIELD EMISSION MICROSCOPY*

By Robert Gomer

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Introduction

The field emission microscope was invented by the German physicist E.W. Müller[1] in 1936 and was developed by him in the following years. The device began to attract general attention only after the end of World War II, and its potentialities are now being recognized. The field and ion microscopes, in conjunction with recently developed ultrahigh vacuum techniques, are rapidly becoming important tools for the study of physical and chemical surface phenomena.

This paper will be devoted to a brief discussion of the theory of the field and ion microscopes, followed by some examples drawn from our current research.

Theory

The existence of very high electric fields at the surface of a metal has long been known to give rise to cold emission of electrons. The phenomenon is a quantum mechanical one and can be explained by reference to the schematic potential energy diagram of FIGURE 1. Electrons may be assumed to fill the flat-bottomed potential energy trough representing the metal (on the left of the vertical line in FIGURE 1) to the height of the Fermi sea, μ. In the absence of external fields, the potential outside the metal is also constant but higher than in the metal by the amount $\mu + \chi$, χ representing the energy difference between a free electron of zero momentum and one at the top of the Fermi sea in the metal. It is equal to the thermionic work function. In the presence of an applied field, the potential outside the metal takes the form shown in FIGURE 1. Electrons in the metal are now confronted by a barrier of finite height and thickness, so that tunneling can occur. The penetration coefficient α for a barrier of this kind is approximately

$$\alpha = \text{const.}\ \exp\left(-m^{1/2}/\hbar \int_0^l \sqrt{V-E}\ dx\right) \qquad (1)$$

where m = electron mass; V = potential; E = kinetic energy and $\hbar$ = Planck's constant 2π. The exponent is seen to be proportional to the area under the curve traced by the square root of the ordinate $u = V-E$ in FIGURE 1. Only electrons very near the top of the Fermi sea contribute to emission, so that E may be replaced by μ. The area in the exponent is

*This paper, illustrated with lantern slides, was presented at the meeting of the Section on November 9, 1954.

nearly triangular and hence given sufficiently well by $\frac{1}{2}\chi^{\frac{1}{2}}l$. However, $l = \chi/F$ so that the field emission current is given by

$$I = \text{const. } \exp-(k\,\chi^{3/2}/F) \qquad (2)$$

F being the applied field in volts/cm. and k a constant of numerical value 6.8×10^7. This simplified derivation[2] shows the correct exponential dependence of field emission current on work function and applied field. A more rigorous derivation leads to the Fowler-Nordheim equation,[3] whose exponential part is identical with that of equation (2).

The field emission microscope utilizes the phenomenon of cold emission as follows. A wire etched to a very fine point ($r \sim 10^{-5}$ -10^{-4} cm.) is surrounded by a spherical anode, usually in the form of a fluorescent screen. The system is evacuated and the metal wire heat polished electrically. FIGURE 2 shows a schematic diagram of a microscope assembly. If a voltage of the order of 10^4 volts is applied between tip and screen, cold emission occurs, since the field at the tip is given by $F \sim V/5r$, so that F will be on the order of 10^7 -10^8 volts/cm. Electrons leave the tip with very low initial velocities, so that they will follow paths parallel to the lines of force, at least initially. Since the latter enter the metal tip perpendicular to its surface, electron paths such as those shown in FIGURE 3 result, producing an electron emission map of the tip, magnified by the amount $D/\delta = x/r$. Thus magnifications of the order of 10^5 to 10^6 are possible.

A typical emission pattern from a nickel tip is shown in FIGURE 4. It will be noticed that it displays the symmetry characteristic of a cubic crystal oriented along its 111 direction. In general, the tips obtained by etching fine wires are of dimensions (10^{-5} - 10^{-4} cm.) much smaller than the average grain size of the crystallites composing the original wire.

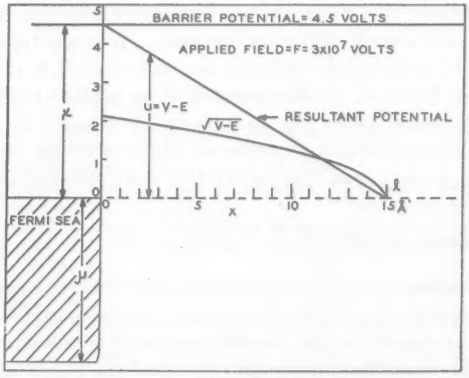


FIGURE 1. Schematic potential diagram for metal surface with applied external potential. Clean metal, no image potential assured. χ represents work function, μ the depth of the Fermi sea.

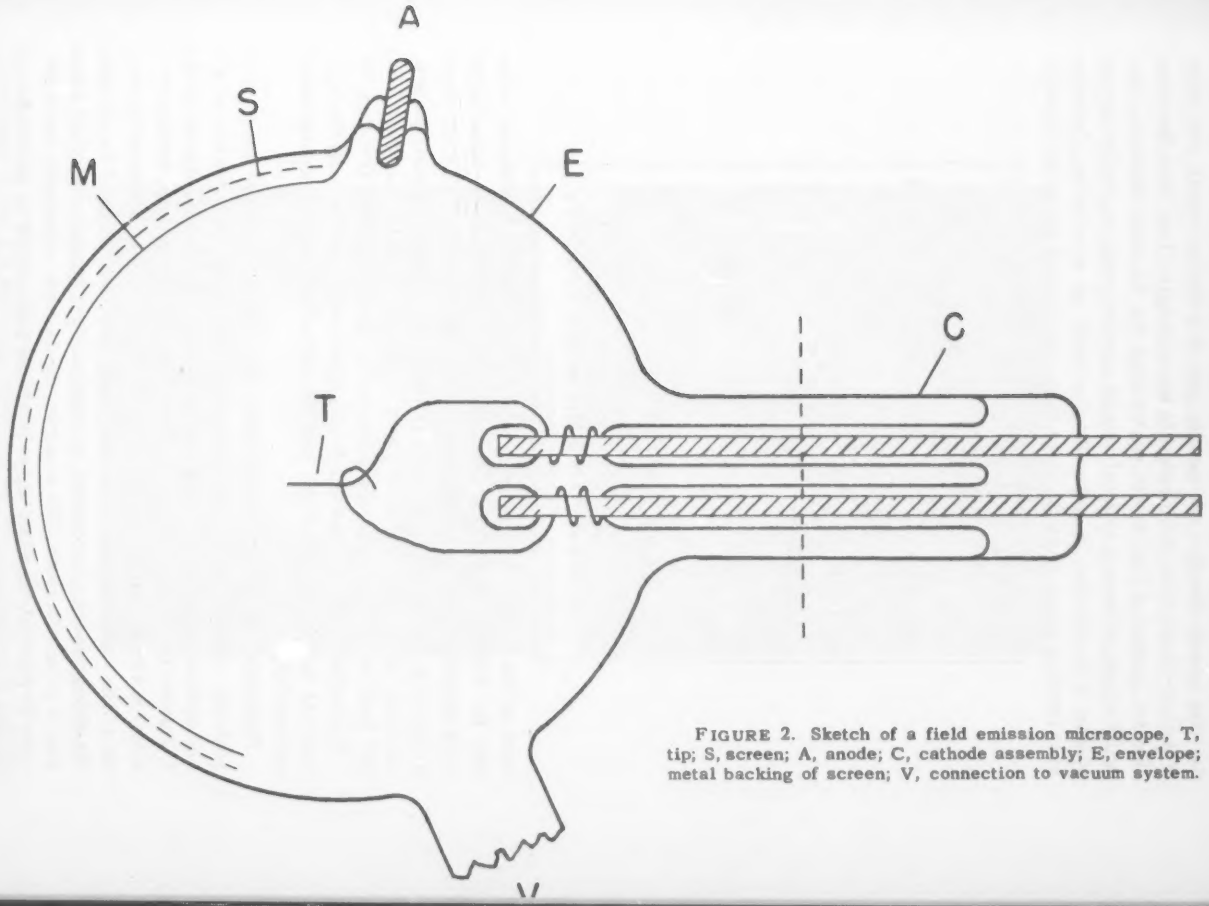


FIGURE 2. Sketch of a field emission micrsocope, T, tip; S, screen; A, anode; C, cathode assembly; E, envelope; metal backing of screen; V, connection to vacuum system.

For this reason, the tips are usually part of a single crystal, very often of definite orientation with respect to the wire axis. If the work functions of every portion of the crystal constituting the tip were identical, one would simply observe a uniformly bright circular patch on the fluorescent screen. Work function is generally dependent on orientation, however, even for clean metal crystals, since it is a function of the atomic arrange-

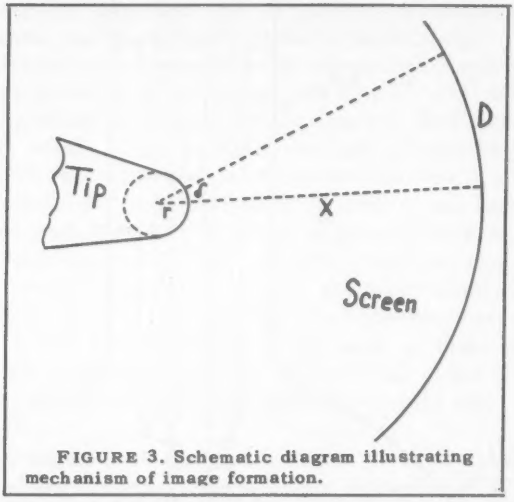


FIGURE 3. Schematic diagram illustrating mechanism of image formation.

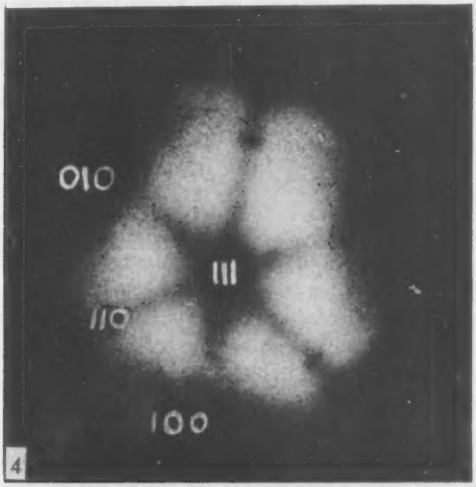
ment at the surface.[4] Therefore one sees a map showing different intensity for different faces. In general, closely-packed faces have higher work functions than loosely-packed ones. In the Ni crystal (f.c.c.) shown in FIGURE 4, the 100, 110, and 111 faces are the most closely packed, and they appear darker than the rest of the tip. In tungsten (b.c.c.) the 110 face is the most closely packed and appears darkest (FIGURE 5)

Crystallographic indices can be assigned unequivocally from the symmetry of the observed patterns and the calculated angular separations of various faces.

Differences in work function occur not only on different faces of a perfectly clean metal crystal but result also from the presence of adsorbed films, or monolayers of gases. Since the work function appears to the 3/2 power in the exponent of the Fowler-Nordheim equation, increases of a few hundredths of an electron volt produce a marked decrease in emission. The field emission microscope may, therefore, be used for studying the adsorption of gases on metal substrates. Most strongly adsorbed gases form a polar adsorbate. The resulting dipole layer contributes an increment to the work function which is almost proportional to the number of dipoles (*i.e.* ad-atoms) per unit area. Thus a calculation of the work

function (in the case of field emission with the aid of the Fowler-Nordheim equation) permits an estimate of the coverage.

Increases in emission may also be caused by local enhancement of the electric field by small protuberances on some portion of the tip. These protuberances tend to act as if they were independent emitters, that is,



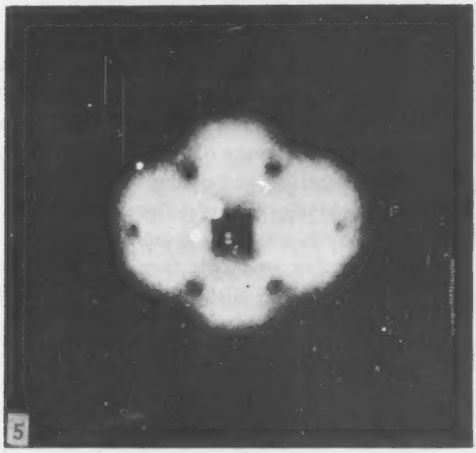


FIGURE 4. 111 oriented field emmission pattern of nickel (crystallographic directions indicated).

FIGURE 5. Field emission pattern of clean tungsten. The central dark region corresponds to the 110 direction, surrounded by four 211 faces; two phthalocyanine molecules are visible (in the center and at the edge of the 110 face).

there will be additional divergence of electrons emitted from them, with the result that the local magnification may be increased by a factor of 10 to 30 over that corresponding to the radius of curvature of the tip as a whole. It must also be remembered that ordinary concepts of conduction and insulation fail at the high field strengths involved, so that even organic dyes transmit electrons perfectly well. These facts make it possible to observe emission patterns corresponding to individual molecules of relatively small size (10 Å.) and to observe incipient overgrowth and surface phases.

Before discussing some experiments along these lines, it is necessary to consider the resolution obtainable with the field emission microscope.[5] It can be shown that the statistical distribution of electron velocity perpendicular to the direction of emission is the factor ordinarily limiting resolution. This velocity, on the average, corresponds to transverse energies of the order of 0.1 e.v., resulting in a limiting resolution of 20 to 30 Å. Detailed study shows that the transverse velocity increases as the square root of the applied field. Since the transit time of electrons from tip to screen varies inversely as the square root of applied voltage and, since the latter is proportional to the field, resolution is essentially independent of the applied voltage. It does increase, however, with decreasing tip size, but is independent of tip to screen distance. The reason is that spreading of the image (*i.e.* increased magnification) without increase in transit time results from a decrease in tip radius but not from an increase in tip to screen distance.

Molecular Images

Perhaps the simplest surface phases observable are the images of individual molecules. These phases were first noted by Müller,[1] who found that the sublimation of small amounts of organic compounds gave rise to small patterns, randomly superimposed on the substrate pattern. FIGURE 5 shows the images of two phthalocyanine molecules on a tungsten tip. Unfortunately there is no one-to-one correspondence between the shape of the patterns and the molecules producing them. This phenomenon is connected no doubt with the distortion of the molecules by the strong electric fields at the surface, insufficient resolution, and the fact that the shape of the patterns may depend more on the form of the electron orbitals than on the geometrical configuration of the molecule. Nevertheless one can utilize the phenomenon for the estimation of the ionization potentials of the adsorbate. Thus the emission from individual zinc phthalocyanine molecules was measured photometrically as a function of voltage, permitting an estimate of the "work function," *i.e.* ionization potential. It could also be shown that the total current emitted by a molecule was in good agreement with theoretical estimates.[6]

Surface Phases

It has been noted that very small surface proclivities can give rise to sufficient field enhancement and magnification to become visible. The study of the initial formation of oxide, carbide, etc. layers is therefore a a fruitful one for the emission microscope.

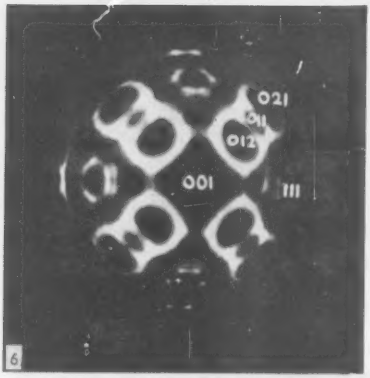
Thus it was found that small amounts of oxygen, carbon, or silicon on Ni tips tended to form definitely oriented (epitaxed) surface phases. FIGURES 6, 7, and 8 show overgrowths of Si, C, and O, respectively. In the case of silicon, it was found that surface precipitation depended on the temperature at which the tip was kept and could be made to proceed in reversible stages.[7] Perhaps the most significant result is that surface precipitation occurs at concentrations and temperatures well below the formal bulk solubility. This inference probably means that the formation of a surface phase corresponds to a decrease in free energy of the system. By heating a silicon-containing tip to a temperature where surface precipitation occurred slowly, it was possible to show that different crystallographic sites accept silicon (or nickel silicide?) in definite order of preference. When the most favorable sites are occupied, the next best regions are filled and so on.

Similar conclusions were reached for carbon and nickel oxide on nickel. These findings probably explain certain cases of catalyst poisoning in which minute traces of impurities inhibit activity. Thus processes occurring only on specific crystallographic sites may be forestalled by the formation of surface phases on those particular locations.

Chemisorption and Mobility of Oxygen on Tungsten

One of the most fundamental properties of chemisorbed layers from the standpoint of catalysis is their mobility. It is clearly of paramount importance in determining the mechanism of a catalytic reaction, or merely in knowing how adsorption occurs to have some idea whether adsorbed atoms remain rigidly fixed on their initial adsorption sites, or whether they are free to migrate over the surface and, if so, at what rate. Up to now conventional methods have been rather unsuccessful in elucidating this phenomenon, since the interpretation of adsorption isotherms is hopelessly obscured by surface heterogeneity and ignorance of the physical condition of the surface.

The field emission microscope offers a very clear-cut and basically simple method of determining the mobility of adsorbates quantitatively. If it were possible to evaporate the gas under study from some suitable source (*e.g.* a heatable copper oxide filament in the case of oxygen) in such a way that only one part of the field emitter would become contaminated, one could determine how, and at what temperature of the tip, spreading occurred. If one attempted to evaporate from a gas emitter placed



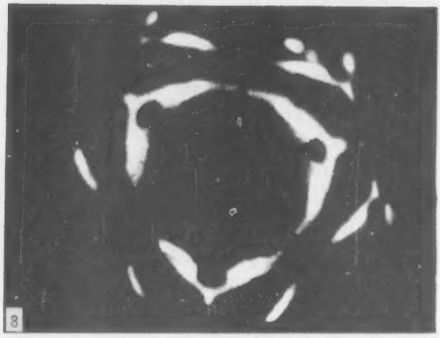


FIGURE 6. Surface phase of silicon on a 100 oriented nickel tip (crystallographic directions are indicated).

FIGURE 7. Surface phase of carbon on a 111 oriented nickel tip.

FIGURE 8. Surface phase of nickel oxide on a 111 oriented nickel tip.

on one side of the tip while the tube was at room temperature, gas rebounding from the walls would instantly contaminate the tip uniformly and the experiment would prove impossible. If the tube is kept immersed in liquid helium (at a temperature of 4° K), the vapor pressure of all gases except helium is negligibly small (for oxygen the vapor pressure is $\sim 10^{-30}$ mm.Hg) and their sticking coefficients on surfaces very high, so that one may carry out the experiment just outlined. Under these conditions, gas does not return from the walls, and only that portion of the tip directly exposed to the gas emitter becomes coated. It is now possible to heat the tip electrically and to determine its temperature from the simultaneously measured resistance, while keeping the tube immersed in liquid helium. Thus the temperature at which mobility sets in may be determined.

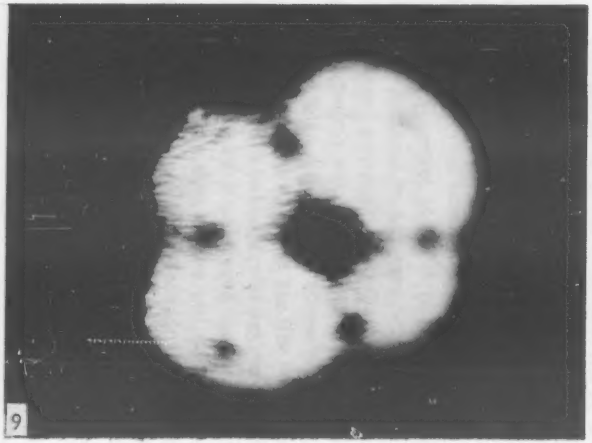
Results obtained with oxygen on tungsten[8] show that, if the amount of

gas evaporated is so small that a monolayer or less is formed on the portion of the tip receiving it, mobility does not set in below 450° K. The phenomenon is clearly one of activated diffusion increasing in rapidity with increasing temperature and not a two-dimensional melting. FIGURE 9 shows a clean tip, FIGURE 10 the same tip after evaporation, and FIGURE 11, the changes occurring after heating to 500° K. It will be noted that the areas initially exposed to oxygen evaporation appear dark before the tip has been heated, but then come up in intensity as the oxygen diffuses out of them into the rest of the tip. It must be noted that this process does not lead to a random contamination. Successive dosing and spreading enable one to ascertain the order in which various crystallographic planes hold oxygen. By varying the temperature of spreading and noting the times involved, the activation energy for the surface diffusion in the monolayer was found to be *ca.* 30 Kcal. This value is a surprisingly high one, and it shows that oxygen is bound very firmly to tungsten.

It could also be shown that these changes occur truly in a monolayer, without involving oxidation of the tungsten substrate, if the temperature of the tip does not exceed 600° K. Above this temperature, oxidation occurs, again with very definitely oriented overgrowth (FIGURE 12). This could be deduced from the fact that patterns resulting from heating above 700° K could not be made to revert to those of a fully covered tip on further exposure to oxygen, while those resulting from heating below this temperature could always be made to assume the appearance and work function of a completed monolayer.

These results hold, as has been stated, for initial loading of the tip to, at most, a monolayer. If oxygen is evaporated in larger amounts onto a tip kept initially at 4° K, totally different behavior occurs. Under these conditions, spreading sets in at 40° K or lower. It is possible to watch the film cover the tip like the unrolling of a carpet or the drawing of a blind, with a sharp, almost uniformly advancing boundary. However, the layer thus formed is not itself mobile; if the amount of oxygen initially evaporated is dosed to "unroll the carpet" only part way, the sharp boundary which is formed will not move until the temperature is raised to $\sim$ 450° K, or until more oxygen is evaporated onto the tip.

These results indicate that oxygen is mobile on an oxygenated tungsten surface at 40° K. The low temperature spreading thus consists of diffusion on top of the already covered regions of the surface; oxygen becomes adsorbed on the clean tungsten at the edge of the layer, making it possible for other molecules to diffuse over the newly covered region. It can be shown from careful work function measurements that the spreading occurs when the coverage is about 80 per cent of the maximum ever observed. This finding indicates that the avidity of the surface for oxygen is sufficiently satisfied, at that coverage, to permit the low temperature



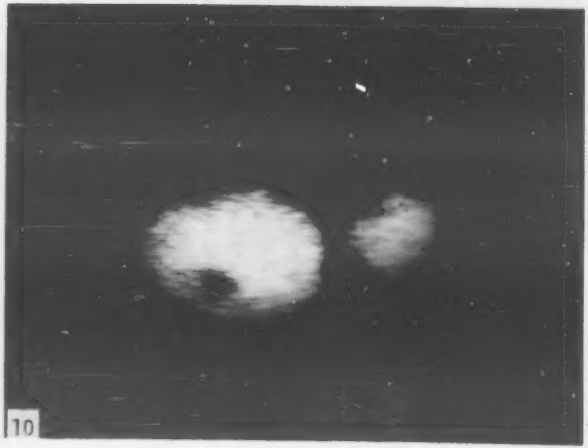
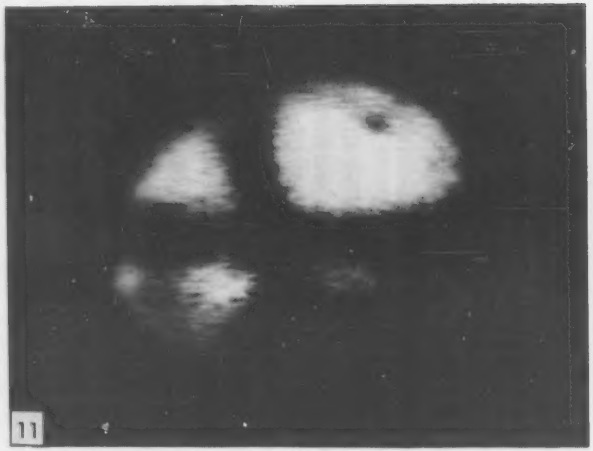


FIGURE 9. Pattern of clean tungsten tip, photographed through liquid helium.
FIGURE 10. Same tip after oxygen evaporation on one side (the oxygen-covered region appears dark).



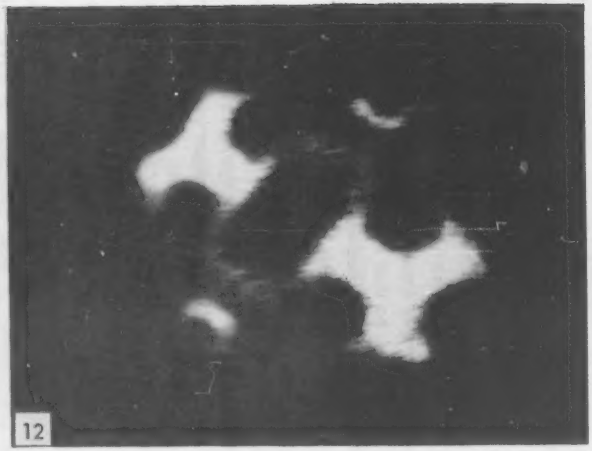


FIGURE 11. Same tip after heating to 500° K. (oxygen has diffused out of the 111 and partly out of the 100 region into the zone 211-110).

FIGURE 12. Oxidation pattern of tungsten, resulting from heating an oxygen-covered tip to 1200° K.

molecular diffusion in the second layer. The results also show that most of the formation of the chemisorbed layer thus occurs very quickly at even the lowest temperatures.

It may be worthwhile to point out that the advantages of liquid helium for the attainment of high vacuum are tremendous for this kind of work. The technique is the only one permitting the speedy attainment of high vacuum after exposure to any gas except helium without the necessity of brute force outgassing methods, so that baking out at elevated temperatures with consequent changes in the chemisorbed layer under study is obviated.

Mass Spectrometric Analysis of Field-Produced Ions

The desorption of positive ions from metal surfaces by high electrostatic fields (~ 1 volt/Å.) was first noted by Müller, who utilized the phenomenon in the so-called field ion microscope.[1] This device consists of a field emission microscope filled with hydrogen gas to a pressure of *ca.* 10^{-3} mm. of mercury. When the tip is made the anode, a faint but sharp image is seen at fields of the order of 2 volts/Å, attributed by Müller to protons desorbed from the surface.

The present work was initially started with the objective of determining the nature of the ions formed in the case of hydrogen, and conceivably of other gases. The field ion microscope now promises to become a tool for studying adsorption phenomena; it also promises to have interesting analytical applications.[9]

A field ion microscope was modified by the introduction of a small hole in the screen through which a beam passed into a mass spectrometer equipped with an electron multiplier as the ion detector. Currents as low as 10^{-19} amperes could be measured in this way. In most experiments an area of about 900 Å.[2] at the edge of the 110 plane of the tungsten tip was "seen" by the mass spectrometer. The peak width corresponding to monoenergetic 10,000 volt ions was 20 volts, permitting resolution of ions formed within 3 Å. of each other.

It was found that ions could be obtained from all of the gases tested. TABLE 1 is a summary of the gases studied and the ions observed. The general nature of the resolved ion current versus field plots shows an increase in ion current as the field is increased, approaching a maximum and thereafter decreasing.[1] Details vary with tip condition, temperature, and pressure. The peak shapes observed in the mass spectrometer demonstrate that ions are formed farther from the tip as the field is increased. The width accounts for the apparent decrease in peak heights observed as fields were raised above 6 volts/Å. The electrostatic fields at which ions were observed in this work were roughly 2.5 times those used by Müller. The probable explanation of this fact is that emission from the 110 plane (exclusively investigated here) is very much lower than that from other planes in tungsten, so that an over-all image would result at

TABLE 1

Parent Gas	*Field*	*Ions observed (primary)*	*Ions observed (secondary)*
H_2	5.6 v/Å	$H^+(0.5), H_2^+(0.5)$	
D_2	5.6 v/Å	$D^+(0.5), D_2^+(0.5)$	
O_2	5.6 v/Å	$O_2^+(1.000)$	
C_2H_6	3.6 v/Å	$C_2H_6^+(0.8)$,	
		CH_3^+ (or $C_2H_6^{++}$) (0.2)	$C_2H_5^+$, $C_2H_4^+$, $C_2H_2^+$
N_2	5.6 v/Å	$N_2^+(1.000)$	
C_2H_4	3 v/Å	$C_2H_4^+(1.000)$	$C_2H_3^+$, $C_2H_2^+$, C_2H^+
CH_4	3 v/Å	$CH_4^+(1.000)$	CH_3^+, CH_2^+, CH^+, C^+
CH_3OH	3 v/Å	$CH_3OH^+(0.7)$, $CH_3O^+(0.3)$	

fields too low to give appreciable emission from the 110 plane. There are several interesting facts to be noted about these results:

(1). All peaks broaden with increase in field, showing that autoionization occurs at sufficiently high fields.

(2). Major peaks broaden asymmetrically, showing sharp cutoff on the high energy side.

(3). In the case of CH_3OH the CH_3O^+ peak broadens much less rapidly than the CH_3OH^+ peak, indicating that the former ion is formed on the surface, and hence probably represents a case of fragmentization due to substrate action.

(4). Fractional mass peaks of low relative intensity occur. These peaks show symmetrical broadening. Two types of these secondary peaks may be distinguished: (a) peaks showing a normal linear pressure dependence; and (b) peaks showing a dependence on the square of the pressure. It can be shown that (a) corresponds to ions which break up due to vibrational excitation (metastable ions). The ions of type (b), on the other hand, correspond to ions which break up collisionally after full acceleration. It is interesting to note that the abstraction of one or more H atoms from hydrocarbon ions seems to be almost equally probable. This phenomenon, of course, is connected with the very high energy of the fully accelerated ions.

Since there are no appreciable mass effects, ion emission can hardly be a tunnel phenomenon of the ions themselves. It is probable, however, that electron tunneling is involved. FIGURE 13b shows the deformation of the potential curves of an electron in an H atom by an external electric field. It is seen that tunneling, *i.e.* autoionization, can occur at sufficiently high fields. FIGURE 13a shows the same atom near a metal surface. It will be seen that the superposition of the image potential

curve of the metal and the atom's potential result, for the same applied field, in a barrier considerably smaller than that at a free atom. Thus one may expect field ionization near a metal surface to occur at fields where pure autoionization is still negligible.

Barrier penetration probably accounts for the loss of one electron in the production of ions from physically adsorbed molecules or atoms, and for the loss of the second bonding electron in the case of species chemisorbed by means of electron pair bonds. The first electron, however, can be removed only when $F > H/a$, where H is the heat of binding, and a the mean electron-to-surface distance. This places a sharp limit on F, since the required potential difference must occur within about one Å. of the surface. If the product F.a is large enough to raise the electron above the Fermi level of the metal, one electron will tunnel. Vibrational excitation of the absorbate will then insure its moving to a distance where the situation of the foregoing paragraph applies. The loss of either bonding electron could be rate controlling. It is probable that the failure to detect O^+ ions from O_2 is due to the high heat of binding of O atoms on tungsten. At fields where O^+ would be produced all O_2 is ionized before reaching the surface.

The technique employed here promises many applications:

(1). Insight into fragmentization processes occurring on metal catalyst surfaces. The use of microsecond pulses should make it possible to resolve the ion beam into the fractions corresponding to each of the three ion-producing mechanisms. To a lesser extent, this objective may be accomplished by increased resolution.

(2). Analysis of gaseous mixtures. Since the parent ions can be made

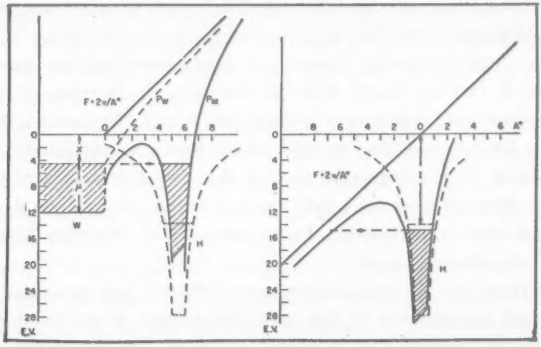


FIGURE 13. Diagrams showing proposed mechanism for field ionization. (a) ionization of physically adsorbed hydrogen atom. (b) pure autoionization. F, applied field; W, tungsten metal; H, hydrogen atom; P_w, image potential, distorted by field; P_m, proton electron potential distorted by field; I, ionization potential of H atom; x, work function; μ, Fermi level; dotted lines represent H potential in absence of metal and field.

to predominate overwhelmingly by using low gas pressures, low fields, and poisoned tips, the present technique avoids the break-up of large and complex ions always concomitant upon electron impact methods of forming ions.

(3). As a tool for measuring the lifetimes of free radicals, photo-excited atoms or molecules, and so on. Since the tip detects the parent, radicals should be simply and unambiguously assigned.

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*SECTION OF MATHEMATICS AND ENGINEERING**

SEARCHING FOR OPTIMUM CONDITIONS**

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Introduction

One question confronting many scientists and engineers (particularly chemists and chemical engineers) is simply, "What combination of the process variables will yield a maximum or optimum response?" Recently, G. E. P. Box and others have proposed techniques and new experimental designs to assist the research worker in solving this very general type of problem.

We begin by imagining that a response y (such as yield, purity, *etc.*) is a function of several independent, controllable, scalar variables (such as temperatures, concentrations, *etc.*). Most generally, this relationship may be written

$$y = f(x_1, x_2, \cdots x_k)$$

where y is the response and $x_1, x_2 \cdots, x_k$ are k continuous variables. We now define a k dimensional system as one in which there are k continuous controllable variables. One or more response variables may be recorded simultaneously. In the case of $k = 2$, the particular response function may be viewed geometrically as a response surface and may be illustrated using a contour diagram. Thus, imagining that yield is a function of temperature and pressure, the contours of yield might take the forms shown in FIGURES 1, 2, and 3. More complicated response surfaces can be imagined, although these simple forms are those usually encountered. For $k = 3$ it is possible to construct contour surfaces, and it is, of course, possible to extend contour representation to any number of dimensions.

Finding a Unique Maximum

Let us now pose a problem frequently encountered by the research worker. Initially, little or nothing is known about the nature of the response surface in the region of interest, and the research worker is out to find that unique combination of conditions which will produce a maximum yield. Several techniques are available to this research worker in his search for an optimum point:

(1) Mapping, *i.e.* observing the response at the intersections of a grid.

*The Section of Psychology held a meeting on November 15, 1954 at which a paper entitled "The Neutral Theory of Personality: New Evidence and a Revision of the Argument" was read by N. Jenkins. This paper will be published in a subsequent issue.

**This paper was presented at the meeting of the Section on November 19, 1954.

(2) An intuitive attack where he proceeds to "hunt and peck" throughout the factor space in the hope of eventually gravitating to the maximum.

(3) The method of "One factor at a time," where all variables save one are held constant, and that single variable is varied until the response is maximized. Then repeating with a second variable, and so on.

(4) The technique of steepest ascent, where, by using a first order experimental design or a simple 2^k factorial design, in some limited sub-section of the experimental region, a path is predicted leading to the maximum point.

The mapping attack may be quickly eliminated as a useful procedure because of the large number of experimental points required. The intuitive attack is admittedly haphazard, may never succeed and, at best, requires a great number of experiments. The method of one factor at a time, although a profitable technique should the surface be a simple mound, as shown in FIGURE 1, will not always direct the experimenter to the maximum. In FIGURE 2, for example, if the response is measured on a path running north-south, the point *A* will be taken as the maximum. If from *A* the experimenter now moves east or west, he will encounter a decline in response. The experimenter may then mistakenly conclude he is at a unique maximum, when, in fact, he is on the crest of a ridge.

The method of steepest ascent provides a program of experiments which *simultaneously* varies the levels of all the independent variables. This predicted path lies along a line normal to the contour lines in the region of experimentation. By observing the change in response along this path, and predicting a new path when necessary, the experimenter moves through the factor space to the maximum point regardless of the nature of the response surface.

To predict the path of steepest ascent, it is essential first to estimate the best fitting planar surface in some restricted region; that is, fit the mathematical model

$$y = \beta_0 x_0 + \beta_1 x_1 + \beta_2 x_2 + \cdots + \beta_k x_k + \epsilon. \qquad (1)^*$$

Provided the observations are recorded at a sufficient number of different levels of the controlled variables, the unknown coefficients in the model may be estimated by least squares. Using matrix notation, we may write the model as

$$\mathbf{Y} = \mathbf{X}\,\mathbf{B} + \epsilon \qquad (2)$$

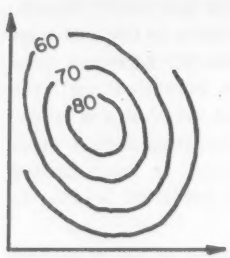
where $\mathbf{Y}$ is an $(N \times 1)$ vector of observations

$\mathbf{X}$ is an $(N \times L)$ matrix of independent variables (L is the number of coefficients in the model)

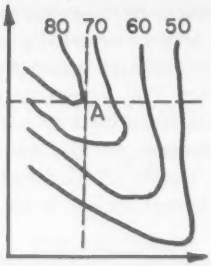
$\mathbf{B}$ is a $(L \times 1)$ matrix of coefficients

and ϵ is an $(N \times 1)$ vector of errors assumed to have a mean of zero and a finite variance.

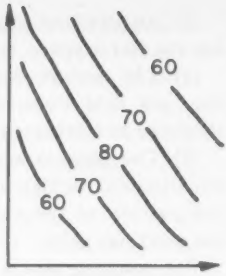
*The variable x_0 is a dummy variable, always equal to unity, and placed in the model to simplify the calculation.



MOUND
FIGURE 1



RISING RIDGE
FIGURE 2



SIMPLE RIDGE
FIGURE 3

The least-squares estimates of the coefficients (the minimum variance estimates) are given by

$$\mathbf{B} = [\mathbf{X}'\mathbf{X}]^{-1}\ \mathbf{X}'\mathbf{Y} \tag{3}$$

The variances and co-variances of these estimates are given by

$$[X'X]^{-1}\ \sigma^2 \tag{4}$$

and an unbiased estimate of σ^2 is given by

$$s^2 = \frac{1}{(N-L)}\ [\mathbf{Y}'\mathbf{Y} - \mathbf{B}'\mathbf{X}'\mathbf{Y}] \tag{5}$$

To facilitate calculations, the actual levels of the controlled variables are first coded to a new set of variables which are called the design variables. The coordinates of the points making up an experimental design are then conveniently listed in a design matrix. For example, the design matrix for a 2^3 factorial, and for a three-dimensional first-order design are given below:

x_1	x_2	x_3
−1	−1	−1
1	−1	−1
−1	1	−1
1	1	−1
−1	−1	1
1	−1	1
−1	1	1
1	1	1

2^3 factorial (cube)

x_1	x_2	x_3
1.414	−.816	−.577
−1.414	−.816	−.577
0	1.632	−.577
0	0	1.731

3-dimensional, first-order design (tetrahedron)

It will be noted that the points composing the 2^3 factorial are the vertices of a cube and, for the first-order design, the vertices of a tetrahedron. Both classes of designs may be extended to k dimensions.

In order to estimate the unknown coefficients, it is first necessary to invert the matrix $\mathbf{X}'\mathbf{X}$. Both the 2^k factorial and first-order designs yield

diagonal $\mathbf{X}'\mathbf{X}$ matrices. In fact, $\mathbf{X}'\mathbf{X} = N\mathbf{I}$ where $\mathbf{I}$ is the identity matrix, and N is the total number of observations. The calculations required for estimating the coefficients are, therefore, quite simple. Furthermore, the variances of the estimates of the different coefficients are all equal.

Thus, the best fitting planar surface is estimated to be

$$y = B_0 x_0 + B_1 x_1 + B_2 x_2 + \cdots + B_k x_k \tag{6}$$

where y is the predicted value of the response and the B_i are the estimated coefficients.

We note now that these estimated coefficients are also the estimates of the first partial derivatives of y with respect to the x_i. They therefore describe the change in y that can be expected for unit changes in x_i. If the independent variables are varied proportionally to their derivatives, we may expect, therefore, the most rapid change in the response. In fact, we have thus determined the orientation of the path of steepest ascent, a path which is normal to the contour lines. For example, if the fitted plane were $y = 15 + 3x_1 - 7x_2$ then, to effect the greatest change in the response y, for every $+3$ units change in x_1 the variable x_2 should be changed -7 units.

Surface Fitting

Oftentimes, however, a system does not possess a unique maximum, or the maximum point may be found to lie beyond the limits of the experimenter's equipment. Furthermore, the experimenter usually does not want an optimum with respect to a single response, but rather wishes to find a region simultaneously optimum with respect to yield, side products, costs, *etc.* If, for example, the yield response were a mound, as illustrated in FIGURE 1, and the cost response the stationary ridge shown in FIGURE 3, then an optimum region could be located by superimposing the two contour diagrams and noting where the minimum cost contour intersected the contour of highest yield. In this fashion, points or regions of interest could be found in the factor space which were optimum with respect to several response variables. What is needed, therefore, is an experimental technique which will estimate the nature of these response surfaces.

Box and Wilson (1951) suggested that a second-order model (illustrated for $k = 2$) be fitted to the experimental data

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_{11} x_1^2 + \beta_{22} x_2^2 + \beta_{12} x_1 x_2 + \epsilon \tag{7}$$

It will be noted that this second-order equation is that of a general conic, and hence for constant y and various values of the B's, the model will estimate contour lines in the form of circles, ellipses, hyperboles, slightly curved or even straight lines. Thus, it is most valuable for estimating the nature of contour systems such as those illustrated in

FIGURES 1, 2, and 3. Furthermore, for $k>2$ this second-order mathematical model can be extended quite simply.

The question immediately arises, at what points in the factor space should the response be recorded so that the second-order model may be properly estimated? The array of points making up such an experimental design should (1) provide estimates of all the coefficients in the model; (2) provide estimates with a desirable variance structure; (3) provide a valid estimate of the experimental error; (4) require simple calculations; and do all this with the fewest possible number of experimental points.

One experimental design which will permit the estimation of all the coefficients in a second-order model is the 3-level factorial design (the 3^k factorial). For the 3^2 factorial the design matrix **D** and **X**, the matrix of independent variables associated with the second-order model, are given below:

$$\mathbf{D} = \begin{array}{c} \begin{matrix} x_1 & x_2 \end{matrix} \\ \begin{bmatrix} -1 & -1 \\ 0 & -1 \\ 1 & -1 \\ -1 & 0 \\ 0 & 0 \\ 1 & 0 \\ -1 & 1 \\ 0 & 1 \\ 1 & 1 \end{bmatrix} \end{array} \qquad \mathbf{X} = \begin{array}{c} \begin{matrix} x_0 & x_1 & x_2 & x_1^2 & x_2^2 & x_1x_2 \end{matrix} \\ \begin{bmatrix} 1 & -1 & -1 & 1 & 1 & 1 \\ 1 & 0 & -1 & 0 & 1 & 0 \\ 1 & 1 & -1 & 1 & 1 & -1 \\ 1 & -1 & 0 & 1 & 0 & 0 \\ 1 & 0 & 0 & 0 & 0 & 0 \\ 1 & 1 & 0 & 1 & 0 & 0 \\ 1 & -1 & 1 & 1 & 1 & -1 \\ 1 & 0 & 1 & 0 & 1 & 0 \\ 1 & 1 & 1 & 1 & 1 & 1 \end{bmatrix} \end{array}$$

Although the 3^k factorial designs do permit the quick estimation of all the coefficients in a second-order model, one major objection to the design is the large number of experimental points required, $N = 3^k$. In addition, the design is an "astigmatic" design. This quality may be described as follows. Once the mathematical model has been fitted, the predicted response y may be obtained at any point in the factor space. The variance of these predicted values may also be estimated at any point. If we define the information at a point to be the reciprocal of the variance at that point, we can then plot contours of equal information. These information contours will be the function solely of the variances of the estimated coefficients and, hence, of $[\mathbf{X}'\mathbf{X}]^{-1}$ which is unique for any particular design. For the 3^2 factorial design the contours of equal information are illustrated in FIGURE 4.

We note now that, as we move in a circle with its center at the center of the design, the information-gathering ability of the design changes. The 3^2 factorial design predicts or "sees" better at a constant distance in some orientations than in others and, hence, we term it an astigmatic design. Since, initially, the experimenter knows nothing about where the optimum region will ultimately lie, it would seem best to use an

experimental design that gathered information equally well in all directions; that is, a design which possesses circular (or spherical) contours of information.

It can be shown that, in order to have circular contours of information (or spherical for $k>2$), the matrix $[\mathbf{X}'\mathbf{X}]$ must have the form illustrated for $k=2$.

$$[X'X] = N \begin{array}{c} \begin{matrix} x_0 & x_1 & x_2 & x_1^2 & x_2^2 & x_1x_2 \end{matrix} \\ \begin{bmatrix} 1 & 0 & 0 & 1 & 1 & 0 \\ 0 & 1 & 0 & 0 & 0 & 0 \\ 0 & 0 & 1 & 0 & 0 & 0 \\ 1 & 0 & 0 & 3\lambda & \lambda & 0 \\ 1 & 0 & 0 & \lambda & 3\lambda & 0 \\ 0 & 0 & 0 & 0 & 0 & \lambda \end{bmatrix} \end{array}$$

where λ is an arbitrary constant, $\lambda > \frac{k}{k+2}$. Arrays of points which

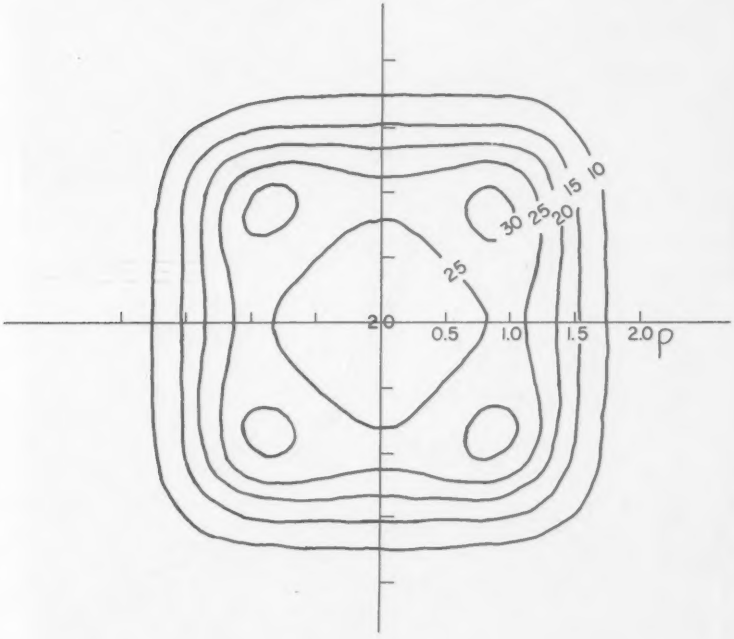


FIGURE 4. Contours of equal information for the 3^2 factorial design (assuming a second degree equation).

would yield this $\mathbf{X}'\mathbf{X}$ matrix (actually the matrix of the moments of the points composing the design) are called rotatable designs. It was found that points lying at the vertices of the regular figures satisfied this criteria.

Rotatable Designs

In two dimensions, an infinite number of second-order rotatable designs are available. The design requiring the fewest points is the pentagonal design, formed from the vertices of a pentagon with an added point at the center. All the 2-dimensional regular figures composed of five or more points generate second-order rotatable designs, provided at least one point is added at the center. Designs combining the pentagon and hexagon, hexagon and septagon, *etc.*, have also been derived.

Points added at the center of the rotatable designs change the information profile of the design. The pentagon design information profile with one, three, and five points at the center is shown in FIGURE 5.

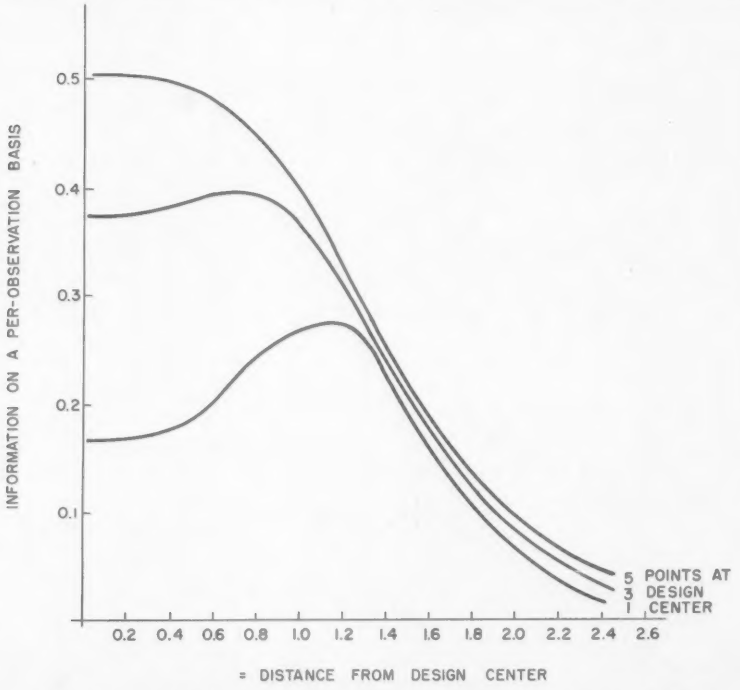


FIGURE 5. Information per observation at a distance ρ from the origin for second order rotatable design with various values of n, where n is the number of points at the center of the design.

It will be noted that, with one point at the center, the information increases as one moves from the center to the circumference of the design but, with three points at the center, the information is almost uniform out to the circumference. This latter design is termed a uniform information design. The distribution of information with five points at the center is identical to that of the normal distribution. Designs of this nature are called orthogonal.

For $k = 3$ dimensions, second-order rotatable designs are provided by the points located at the vertices of the icosahedron and dodecahedron. By combining the cube and octahedron, another design is possible. In every case, at least one point is required at the center of the design. For $k > 3$, second-order rotatable designs are formed exclusively from combinations of the hypercube and measure polytope. In general, $2^k + 2k + n$ points are required for these designs, n being the number of points at the center. Taking advantage of the 2^k fractional factorial designs, the total number of points may be further reduced for $k \geqslant 5$ and again for $k \geqslant 8$. Uniform information and orthogonal rotatable designs are also available in k dimensions.

Canonical Reduction

Once the second-order model has been fitted to the data, the equation can be reduced to its canonical form by the translation and rotation of axis. Thus, the second-order equation (7) may be rewritten as

$$(y - y_0) = A_1 X_1^2 + A_2 X_2^2$$

where y_0 is the estimated response at the center of the fitted system, X_1 and X_2 are the canonical variables and A_1 and A_2 are constants. Whenever A_1 is much larger than A_2, slight changes in X_1 will profoundly affect the predicted response, while corresponding large changes in X_2 are required for an equal response change. In fact, it may be that all practical changes take place along the X_1 axis. If this is so, we have a demonstration of variable redundancy, for what was thought to be a two-dimensional system (x_1 and x_2) can effectively be demonstrated in but one variable, X_1. For large values of $k, (k > 3)$ this phenomenon frequently occurs. Of course, the canonical variables are linear functions of the original controlled variables, *i.e.*

$$X = \sum_{i=1}^{k} a_i x_i$$

and it may be difficult to rationalize physically or chemically for the meaning of these linear functions. However, knowledge of the number of these important variables, and their orientation in the factor space, provides the experimenter with much useful information.

Conclusion

The details of the numerical calculations required for determining a path of steepest ascent, surface fitting, and canonical reduction can be found in references 1, 4, and 5. The material on the rotatable designs appears in mimeograph form in reference 3. The techniques are also described in some detail in the text *Design and Analysis of Industrial Experiments*, edited by O. L. Davies and written by a corps of Imperial Chemical Industries chemists and statisticians. The portion on the techniques described above is written by G. E. P. Box, the progenitor of these methods.

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SECTION OF ANTHROPOLOGY

AN ETHNOECOLOGICAL APPROACH TO SHIFTING AGRICULTURE*

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Methods of shifting cultivation, while unfamiliar to many of us living in temperate latitudes, are typical of vast areas in the tropics. Such methods account for approximately one third of the total land area used for agricultural purposes in southeast Asia today (Dobby, 1950:349). In some countries, including the Philippines, it has been estimated (Pelzer, 1945:29) that shifting cultivation produces food for up to 10 per cent of the total population. In these regions the economy of large segments of the upland population is based solely on such means. Nevertheless, shifting agriculture is still only inadequately understood. It is often categorically condemned as primitive, wasteful, or illegal, with little or no regard for such pertinent local variables as population density, available land area, climate, or native agricultural knowledge. For most areas, detailed field reports against which such statements might be tested are totally lacking. There is a definite need for ascertaining what are the real facts about shifting agriculture.

In this paper, I shall attempt to throw some light on the nature of such methods of upland farming and to draw our attention to certain important problems in this area of research. First we shall review some of the more frequent statements made by writers on the subject. Then we shall examine the pertinent ethnographic data for a specific culture, emphasizing not only the local environmental conditions and their apparent modification, but especially the determination of how these conditions and modifications are culturally interpreted.

For our purposes we may consider shifting cultivation, also known by such designations as field-forest rotation (Pelzer, 1945:17) or slash-and-burn agriculture, as always involving the impermanent agricultural use of plots produced by the cutting back and burning off of vegetative cover. We shall call such a field a *swidden*. This term, like its by-forms *swithen* or *swivven*, is an old dialect word from northern England (Northumberland, Yorkshire, Lancashire, and elsewhere) meaning "burned clearing" or "to burn, sweal, or singe, as heather" (Hallowell, 1847:838; Wright, 1904, 5:881-882). It has been revived recently, and in an ethnographic description, by a Swedish anthropologist (Izikowitz, 1951:7). There are many vernacular terms for swidden, but few are widely known

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or used in the literature except in reference to limited geographical regions: *kaingin* (*caiñgin*) in the Philippines, *ladang* in Indonesia, *taungya* in Burma, and terms (see Pelzer, 1945:16) such as *djum* in India, *chitemene* in parts of Africa, and *milpa* in Central America.

Swidden agriculture, of course, involves more than is stated in our minimal definition, but before we attempt greater precision, let us examine some of the characteristics which various authors have attributed to it. The following list is not intended to be complete, but does include the most frequent and problematic statements and assumptions I have encountered.

(1) Swidden farming is a haphazard procedure involving an almost negligible minimum of labor output. It is basically simple and uncomplicated.

(2) Usually, and preferably, swiddens are cleared in virgin forest (rather than in areas of secondary growth). Tremendous loss of valuable timber results.

(3) Swidden fires escape beyond cut-over plots and destroy vast forest areas. One author states that from 20 to more than 100 times the swidden area itself are often gutted by such fires (Cook, 1921:313).

(4) Swidden techniques are everywhere the same. Such features as the lack of weeding and the use of a single inventory of tools are practically universal.

(5) Stoloniferous grasses such as "notorious *Imperata*" (Gourou, 1953:18) are abhorred as totally useless pests by all groups whose basic economy is swidden agriculture.

(6) Swiddens are planted with a single (predominant) crop. Any given swidden can thus be said to be a rice or a maize or a millet field or the like. Hence, it is possible to gauge the productivity of a swidden by ascertaining the harvest yield of a single crop.

(7) Furthermore, it is possible to gauge the efficiency (*i.e.*, relative to some other method of agriculture) of a given swidden economy in terms of its one-crop yield per unit of area cultivated (Hutton, 1949).

(8) Swiddens are abandoned when the main crop is in. "The harvest ends the series of agricultural operations" (Gourou, 1953:28).

(9) There is no crop rotation in swidden agriculture. Instead, soil fertility is maintained only by the rotational use of the plots themselves. The duration of the rotational cycles can be determined by the time interval between successive clearings of the same plot.

(10) Not only is fertility lost, but destructive erosion and permanent loss of forest cover result from reclearing a once-used swidden after less than a universally specifiable minimum number of years of fallowing (set by some authors at 25 years, *e.g.*, Gourou, 1953:31). It is claimed that "dangerous" consequences of more rapid rotation often result from native ignorance.

On these and many other points there is frequently an over-all as-

sumption that the standards of efficiency in terms of agricultural economy in the United States or Western Europe are attainable and desirable among any group of swidden farmers.

Field Observations

From November 1952 until January 1955 I lived with the Yāgaw Hanunóo of southeastern Mindoro Island in the Philippines. The Hanunóo, numbering approximately 6,000, are pagan mountaineers who occupy about 800 square kilometers of forest and grass-covered hinterland, and whose primary economic activity is swidden agriculture (Conklin, 1953:1-3). I was able to observe and participate in more than a full annual cycle of agricultural activities. Since most of my efforts during this time were directed toward an ethnographic analysis of the relation between Hanunóo culture and the natural environment (Conklin, 1954), I was drawn toward an increasingly closer examination of Hanunóo concepts of the ecology of the Yāgaw area and of Hanunóo methods of swidden farming.

The following brief statements summarize the preliminary results of my investigation of Hanunóo swidden agriculture. Except where otherwise noted, these remarks apply specifically to the Hanunóo on the upper eastern slopes of Mt. Yāgaw (FIGURE 1). The six settlements in this area comprise an unstratified, unsegmented, neighborhoodlike community, which has a total of 128 inhabitants. The average population density for the entire Hanunóo territory is 10 per square kilometer, but in the more heavily settled areas, such as Yāgaw, there are from 25 to 35 persons per square kilometer.

The Hanunóo do not have a general term for swidden or for swidden cultivation, but do employ a set of terms distinguishing developmental stages of potential, actual, or used swidden areas. These are based on changes—natural or artificial—in the vegetational cover. Swidden activities are best outlined by taking these stages in sequence, indicating the significant human activities and plant changes occurring at each:

First year. (1) Activities resulting in a slashed clearing, a *ġāmasun* (January-February): Possible swidden locations are discussed within the settlement group. Final decision depends on location augury, dreams, the local omens, as well as an intimate knowledge of the local forms of vegetation. The cultivator marks his plot with bamboo stakes and, using a large bolo, cuts down the underbrush and small saplings. Men and women participate in this initial clearing, family units making up the usual work teams. The average size of a Hanunóo swidden is two fifths of a hectare. This area averages about one hectare of cultivated swidden cleared each year for every eight people. The total area of productive swidden land in a given area, however, is always several times that of the most recently cleared fields, because of intercropping (see below). As shown in FIGURE 1, 48 new swiddens (numbered serially

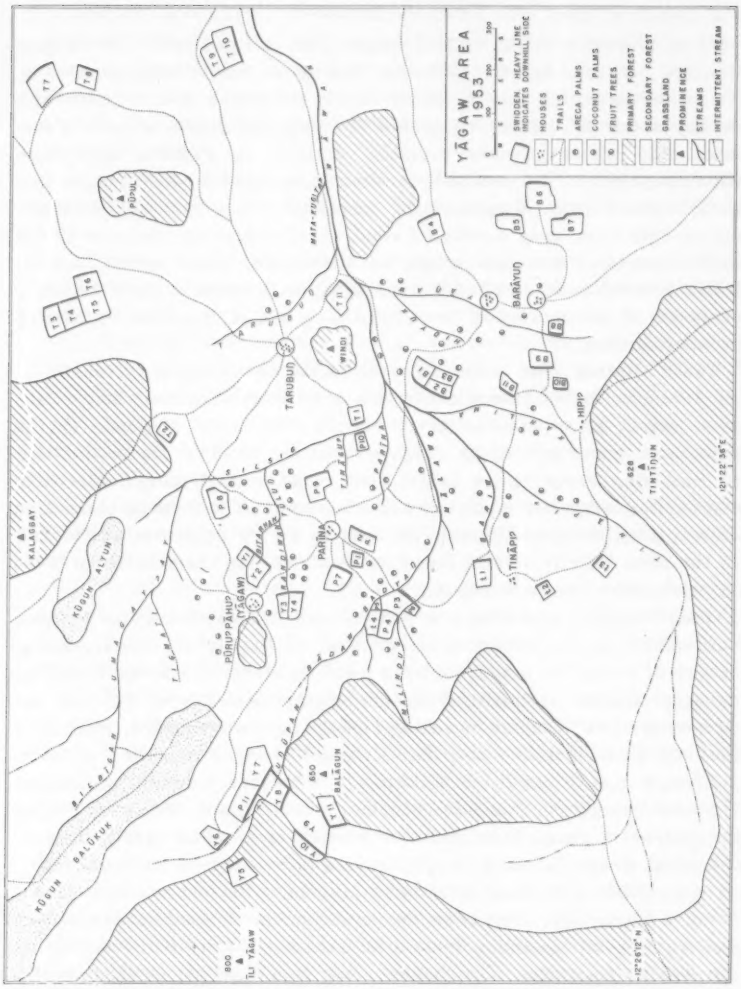

FIGURE 1. Yāgaw Area, Mindoro. (Sources: field data; Coast and Geodetic Survey, 1935.)

for each settlement) were cleared in the Yāgaw area in 1953. Of these only four were cut partly from virgin forest (amounting to less than 10 per cent of the total area cleared). Second-growth forest areas are preferred because the clearing of primary forest requires much more man power for a given area, and demands a longer drying period before burning can take place than can profitably be allotted to such tasks.

(2) Activities resulting in a cut clearing, a *buklid* (February-March): Using the same bolos and a few single-bladed axes, men begin the more arduous task of felling large trees. Women continue to clear undergrowth and begin planting root crops (such as taro) which can survive the intense heat of swidden burning. Instead of being felled, a number of larger trees are pollarded, not only to remove unwanted shade branches, but also to provide firewood and promote seeding of other trees in the first fallow year. Smaller branches and cut underbrush are spread over the whole area so that complete burning will occur and exposed patches of earth will be protected from the dry season sun. These cutting, trimming, and drying activities may take more than a month, especially in a primary forest clearing. Group labor parties, repaid with feasts of rice, are usually needed to finish this stage.

(3) Activities resulting in a burned clearing, a *tūtud* (March-April-May): While the field drys, the Hanunóo farmer removes cut timber suitable for fence building or other construction purposes and clears a 4-meter-wide safety path around the entire clearing to prevent the fire from escaping into surrounding forest or fallow swidden areas. Firing starts at the upward and windward margins. A steep hectare of dry, second-growth vegetation will burn up in an hour or less, depending on the wind. While secondary burning is being completed, men begin fencing the entire swidden to prevent wild and domestic mammals (especially the zebu) from getting at young crop plants. Constant guarding against daytime animal marauders is facilitated by the construction of scarecrows of straw, wind-blown dangling objects, and small field houses from which children can jerk lines leading to distant parts of the swidden.

(4) Activities resulting in, and necessary for the maintenance of, a planted swidden, a *tanman* (May through October): Maize is planted soon after the swidden is burned. The main rice planting comes at the end of the dry season, in May or early June. It is an important social and religious event and involves special spirit offerings, large work parties in gala attire, feasting, and the participation of men, women, and children. Men make the seed holes (ca. 5 cm. deep and 25 cm. apart) with 2-meter long, pointed dibbles. Women and children follow, dropping a small handful of prepared seed (often from a mixture containing small quantities of pigeon pea, cucumber, melon, and sorghum seeds as well as rice). The Yāgaw average for planted rice seed is 40 gantas (1 3/5 cavans) per hectare. Other important swidden crops are planted less

ceremoniously (*e.g.*, sweet potatoes, in August), as are many secondary (*i.e.*, nonstaple) crops. During the rice growing season, other swidden activities include: completion of fences, continued guarding against destructive animals and birds, constant thinning and weeding (the entire swidden area being cleaned of weeds, shoots, and noncultivated vines at least three times), building of granaries, and the almost continuous planting and harvesting of other crops in both new and old swiddens (see discussion of intercropping below).

(5) Activities resulting in a riceless field, a *dayamihan* (October-November): The most important harvest in a new swidden is that of short-growing-season maize (in July and August). This is usually performed (including minor magical rites) by the cultivator himself, with only one or two helpers. The main rice harvest, in late October and early November, involves elaborate arrangements for group labor, feasts, magical rites, and religious offerings. It is the most important agricultural event of the year. Harvesting rice is done by hand (usually without knives) by men, women, and older children. The normal yield in rice ranges from 25 to 40 times the volume of the seed planted. One hectare of swidden land may give more than 30 cavans of unhusked rice. After threshing, drying, hulling, cooking, and other preparations, a settlement-wide celebration is held, after which the rigid observance of many rice-connected taboos, such as that which forbids one to eat new rice from another's swidden, are removed.

(6) Activities resulting in a cleaned swidden, a *lūmun bag·ʔūhan* (November-December): After gleaning, all rice stalks are cut, piled, and burned. Group labor, with compensatory rice feasts, are necessary to finish this task in less than two months. Other cultigens, especially leguminous crops and sweet potatoes, are now the focus of attention.

Dry season swiddens, always cut in second-growth areas, are cleared in September and October, planted in early November, and harvested unceremoniously in February, March, and April. They are usually small and are planted with corn and root crops only, never with rice. Some dry-season crops (including maize, certain beans, and sugar cane) are planted in main swiddens a few weeks before the rice harvest.

After the first year. (7) Activities resulting in a recleaned (used, but still productive) swidden, a *lūmun dāʔan:* Fruit trees, and other perennial cultivates planted in new swiddens continue to provide edible food products if the plot is systematically weeded and cleaned. By interplanting cultigens other than the principal grain staples, the Hanunóo practice a kind of limited crop rotation. Such intercropping results in successive harvests of different primary and secondary crops for at least two years, frequently extended to five or six years, especially where the cultivation of banana plants is continued. The many leguminous crops so interplanted incidentally return significant amounts of

nitrogen to the soil (Wernstedt, 1954:65). Single-crop swiddens are nonexistent. Up to 40 separate crops have been observed growing in one Hanunóo swidden at the same time (*cf.* Anderson, 1952:84; Merrill, 1906:179-180; Hester, 1953:290; Segawa, 1953). One informant drew a map of an "ideal" swidden containing 48 basic kinds of plants (over 250 subsumed specific types) including: 41 cultigen crop foods (including varieties of rice, sweet potatoes, yams, taro, maize, squash, sugar cane, and beans); 1 noncultigen food plant (papaya); and 6 nonfood cultigens, namely: tobacco, for chewing with betel, areca and lime; betel vine, for leaves used in the betel chew; cotton, for spinning and weaving into garments; indigo, for dyeing cotton yarn; derris, for its fish-stupifying roots; and vetiver, for its scented roots (sachet).

Once productive cultivates give out—but usually not for two or three years after the main rice harvest—fallowing begins. After five years, fallow second-growth forest (*talun*) types are readily distinguishable by their predominent plant forms. The most common types are either some kind of tree or bamboo. Bamboo second growth is preferred for swidden making, because it dries uniformly and burns quickly and completely. If not recleared, of course, *talun* eventually reverts to primary forest (*pūru?*). Swidden areas are not recut before at least five years of fallowing—after the last cultigens give out—and this period is extended preferably to more than ten. In 1953, most Yāgaw swiddens had been fallowed for more than eight years. The Yāgaw area is in a rain belt and thus fallowing usually means the growth of replacement forest and a continuing natural refertilization of the land. In areas where there is a long dry season—aided by frequent burning for hunting purposes—tough grasses tend to dominate the replacement vegetation. Without artificial manuring and draft animals, productive swidden cultivation then becomes difficult. Damper areas seem more suited to continued swidden making. Despite an apparently long history of occupation by swidden farmers—there are more than a dozen groves of coconut palms in the area (see FIGURE 1)—the Yāgaw region today includes very little grassland. And *kūgun* (*Imperata* spp.), the predominate grass, is highly valued for livestock pasturage and especially for roof thatching. It is a persistent weed, but in other respects it is an important economic necessity.

Swidden activities require from 500 to 1000+ hours of work per year on the part of the average adult Hanunóo. In addition to swiddens, houseyard gardens are kept for experimentation with new cultigens, and for the individual cultivation of medicinal, ritual, aromatic, and ornamental plants.

The Hanunóo recognize innumerable natural and artificial factors as variables affecting swidden agriculture. Ecologically speaking, climatic factors, while closely observed, can be modified least by the Hanunóo. Edaphic factors, though not practically amenable to artificial change, can be dealt with in a more concrete manner. A study of Hanunóo soil

classification and associated ideas regarding suitability for various crops—other variables being equal—checked well with the results of a chemical analysis of soil samples. Ten basic and 30 derivative soil and mineral categories are distinguished by the Hanunóo farmer. He may not know of the minute degree of lime disintegration and low pH value of *nāpunāpuʔ*, but he does know that certain beans and sugar cane (considered "high lime" crops, technically) will not thrive in such soil as they will in *barəgʔaŋ* (which has a higher lime content and pH value). Effects on soil quality of erosion, exposure, and overswiddening are well understood. They are topics of frequent discussion, and preventive measures are often taken. Biotic factors are most subject to control and experimentation by the Hanunóo, and are of the greatest concern to them. More than 450 animal types and over 1,600 plant types are distinguished. The floral component is the more significant, especially in regard to swidden agriculture. Of some 1,500 "useful" plant types over 430 are cultigens (most of which are swidden-grown), existing only by virtue of the conscious domestication of the Hanunóo. Partly as a result of this intensified interest in plant domestication and detailed knowledge of minute differences in vegetative structures, Hanunóo plant categories outnumber, by more than 400 types, the taxonomic species into which the same local flora is grouped by systematic botanists (Conklin, 1954).

Conclusions

Much of the foregoing is fragmentary and perhaps more suggestive than conclusive. There is certainly a need for continued research in other areas (Leach, 1949:28), and for field observations covering greater periods of time. However, by using what recent ethnographic materials are available, we may tentatively rephrase the statements made earlier, so that a more accurate picture of swidden agriculture will emerge. Most of the changes we shall make indicate that the swidden farmer sometimes knows more about the interrelations of local cultural and natural phenomena than ethnocentric temperate zone writers realize.

(1) Swidden farming follows a locally-determined, well-defined pattern and requires constant attention throughout most of the year. Hard physical labor is involved, but a large labor force is not required.

(2) Where possible, swidden making in second-growth forest areas (rather than in primary forests) is usually preferred.

(3) Swidden fires are often controlled by firebreaks surrounding the plot to be burned. Accidents happen, but greater damage may result from hunting methods employing fire in an area having a long dry season than from swidden clearing *per se*.

(4) Many details of swidden technique differ from area to area, and with changing conditions. Weeding is assiduously accomplished in some

regions. Fencing is considered requisite if domestic cattle are kept, less so where such animals are rare. Wooden hand implements are very simple and are used only once. Metal cutting implements and harvesting equipment, however, vary greatly from region to region.

(5) Even the most noxious weeds, in one context, may serve the local economy admirably in another. *Imperata*, if dominant, restricts swidden opportunities, but its total loss causes similar hardships for those depending on it for pasture and thatch.

(6) Swiddens are rarely planted with single or even with only a few crops. Hence, the productivity of a swidden can be determined only partially by an estimate of the harvest yield of any one crop.

(7) It appears that the efficiency of swidden farming can be ascertained—relative to some other type of economy—only by taking into account the total yield per unit of labor, not per unit of area (Hutton, 1949; Leach, 1949).

(8) Because of intercropping, the harvest of one main swidden crop may serve only to allow one or more other crops to mature in turn. Plantings and harvests overlap usually for more than a full year, and frequently continue for several years.

(9) Swidden intercropping, especially if wet season cereals are alternated with dry season leguminous crops, amounts to a type of crop rotation, even if on a limited scale. Cycles of field "rotation" cannot be meaningfully assessed by merely determining the number of years which lapse between dates of successive clearings. The agricultural use of the swidden plot following initial clearing may have continued for one, several, or many years.

(10) It is difficult to set a minimum period of fallowing as necessary for the continued, productive use of swidden land by reclearing. Many variables are at work. A reasonable limit seems to be somewhere between 8 and 15 years, depending on the total ecology of the local situation. Swidden farmers are usually well aware of these limitations.

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SECTION OF ANTHROPOLOGY

SOME ASPECTS OF TECHNICAL ASSISTANCE IN LIBERIA*

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In this paper I shall discuss a few selected aspects of the early phases of the work of the United Nations Educational, Scientific, and Cultural Organization (UNESCO) Technical Assistance Mission to Liberia, of which I was anthropologist and chief of mission from March 1951 through June 1952. My purpose is not merely to report on the results of the mission. My personal experience has shown that, on such a mission, the role of the anthropologist should be kept apart and distinct from any other functions. While this distinction may, on the surface, appear to be a simple and obvious point, the fact is that, in the discussed situation, the anthropologist was originally asked to perform both administrative and research functions. There may be some advantage in having an anthropologist serve as a head of mission on the assumption that his special knowledge and techniques will somehow pervade the entire program, but only confusion can arise through a misguided effort to utilize the anthropologist both in his professional role as anthropologist and in the role of administrator.

There is a growing body of literature, based on the experience accumulated by anthropologists and others connected with technical assistance programs, that deals with various facets of the problems encountered in this type of work. For the most part, these authors offer generalizations, based on field experience, for the purpose of giving advice and suggestions which would be useful in the execution of future programs. Some examples of these writings have appeared in recent issues of *The Scientific Monthly* (Erasmus, 1954; Jenney and Simmons, 1954; Roney, Jr., 1954) and *Human Organization* (Mandelbaum, 1953; Adams, 1953). These articles are written mainly by individuals who are, or have been, connected with such programs largely under United States Government auspices. Among other recent publications in this field, there are two of importance which have been issued by UNESCO (Mead, 1953; Opler, 1954).

I do not know if there are extant accounts of the problems and operation of particular missions such as I have undertaken to give here. Such accounts should become part of the record in order to augment the generalizations that have been made. While principles and problems may be similar everywhere, no two situations are identical. For anthropologists who, in the future, may also find themselves occupying the dual role of anthropologist and administrator, the present account may be of some value.

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While the literature of technical assistance is dominated, understandably, by attention to the practical problems, there are, at the same time, fundamental theoretical questions which are implicit in sucn programs. For example, what are the basic assumptions behind technical assistance in a world dominated by only a few powers and filled with international tensions generated by struggles for power? What are the ultimate consequences, for the people of underdeveloped areas, of technical assistance programs which are, in essence, things of "shreds and patches" and not the result of a total plan of development in which all social and economic contingencies and eventualities are anticipated and prepared for at least in a general way? A UNESCO document on Cultural Patterns and Technical Change (Mead, 1953) suggests that the cultural integrity of the people receiving such assistance be preserved and that "any planned detail of change" be related to "the central values of the culture." Is it to preserve the cultural integrity of people and respect the central values of their culture as technical assistance programs are planned and executed today under a variety of auspices—the United Nations and Specialized Agencies, the United States Foreign Operations Administration, the Colombo Plan, private foundations, and missionary organizations? It may be well to bear such questions in mind during the course of the discussion which follows.

In accordance with standard procedure for all member governments of the United Nations and its Specialized Agencies, this mission was sent to Liberia by the Technical Assistance Department of UNESCO as a result of a request for aid in the field of education submitted by the Liberian Government. The two parties, Liberia and UNESCO, agreed that an expert should first be sent on an exploratory mission which would result in an agreement for specific kinds of technical assistance. UNESCO was fortunate in securing the services of the late Doctor John Embree for this first phase. During only nine days in Liberia he surveyed the situation and wrote a clear report showing deep understanding of the problems of the country. The agreement for technical assistance was signed on August 14, 1950, before Doctor Embree left Liberia.

The agreement provided for the following personnel and kinds of assistance:

(1) An anthropologist "to aid in studies that are intended to reveal pertinent information about people for whom demonstration schools are planned. To provide cross-cultural data and advice for teachers in schools in non-English speaking areas. To train students in social science techniques."

(2) An educational psychologist "to assist in the development in Liberia of culture-free objective tests. To aid in the development of demonstration schools."

(3) An expert in fundamental education and handicrafts "to develop ways of preserving handicrafts, to work with the anthropologist and with

the Art Department of Liberia College and aid in development of demonstration schools."

(4) Four college science teachers "to develop and teach" mathematics, chemistry, physics, and biology.

(5) A vocational education teacher "to teach in and operate a shop; to be able to integrate field training with classroom theory."

Another condition of the agreement was the provision by UNESCO of seven fellowships for Liberian students in vocational education, general science, special fields of science, and social science. There was also to be a fund of $4500 for the purchase of science equipment, visual aid teaching equipment, and other needed materials.

Before leaving the United States for a briefing and a physical examination at UNESCO House in Paris, I was sent to Washington to confer with various Technical Cooperation Administration (Point 4) officials about the UNESCO program and about the activities of TCA in Liberia in order to establish a basis of cooperation between our programs and to prevent duplication of effort. The important problem of cooperation among the different technical assistance missions cannot be treated here, but in addition to UNESCO and the Technical Cooperation Administration which arrived first in Liberia, there were also teams from World Health Organization, Food and Agriculture Organization, International Labor Organization, International Civil Aviation Organization.

Upon my arrival at UNESCO headquarters, I learned that the four science teachers were already in Liberia, that candidates for the posts in fundamental education and vocational education had been rejected by the government and that new candidates in these fields were being sought. I spent about 10 days talking with individuals in various departments of UNESCO who regretted they could help us little since this mission was among the first to be sent into the field. We learned much about the activities and programs of UNESCO, and considerably less about Liberia. In the course of an interview with the Director-General of UNESCO I was somewhat shaken when he expressed the hope that our mission would have good progress to report in the next three months. He wished to impress the value of such programs on the delegates to the next general conference of UNESCO which was to be held at the end of that period.

Soon after the beginning of our work in Liberia it became clear that my role as an anthropologist was doomed to oblivion. I was compelled, in the interests of the mission, to function solely as its head, which means that I was cast in the roles of administrator, diplomat, and public relations tactician, among others. The government received us hospitably with its very limited facilities, and extended every diplomatic courtesy and prerogative. But it was very difficult for as many as three families to share a house for any considerable length of time, and it proved impossible to meet the transportation needs of six experts with a single car. These conditions, and others, improved in time, but problems of inade-

quate equipment persisted. There was also frustration from a slower tempo of activity than that to which the mission personnel was accustomed. Such problems affected morale and efficiency, and took up a great deal of administrative time.

A major concern was to have our mission accepted by the government with complete confidence. Few officials expected us, or even knew why we were in Liberia, other than the President, the Secretary of State, and some officials of the Department of Public Instruction. Therefore, one of my first tasks was to "educate" the government as a whole on the nature and purpose of our mission. This task occupied a considerable portion of my time during the first few months. It was not sufficient to write explanatory memoranda. Only through personal contact could we convince the authorities of our sincerity, integrity, and dedication of purpose. In the course of events, I discovered that it was of the utmost importance to emphasize constantly that we were engaged in a Liberian Government operation, not just a UNESCO program.

This point was of special importance in connection with my later efforts to achieve some organization in the government for the execution of the fundamental education program, which involved specialized operations in literacy, primary schooling, agriculture, public health, and public works. It was essential, therefore, to enlist the cooperation of the departments of the government charged with those specific functions. As head of mission I was able, after month of work, to create the basis of this cooperation in the Liberian National Commission for UNESCO. A fundamental education expert had not yet been selected, and in this effort I was preparing the ground for his arrival.

The deficiencies of the program, as described in the agreement, soon became apparent. Many of the projects that we were called upon to assist in were vague, diffuse, unplanned, and essentially unsuited to the situation at hand. For instance, it was stated in the agreement, under the heading of Science, that "Liberia College is embarking on a programme intended to provide young Liberian men and women with backgrounds in mathematics, chemistry, and physics" and that to this end a teacher is needed in each of the fields of mathematics, chemistry, physics, and biology. Surely something more was needed than "background" by young men and women in a country where few, if any, professionals in these fields were employed outside the Firestone plantation. We therefore felt that some more practical focus had to be given this project.

One more example, among several that might be cited, will serve to emphasize the point. Under the heading of Education the following project appeared: "Liberia College proposes to establish a school of education for the training of teachers on the college level. In providing students with actual teaching experience, the college, working in cooperation with the Department of Public Instruction, plans to establish in at least three villages, demonstration schools. These schools, it is hoped, will meet

the needs of the total community, providing education for both adults and children. In spite of the fact that this programme will be structured at Liberia College there will be a very real programme inaugurated in the hinterland that is designed to meet the needs of the people at their cultural level."

We found there was no discernible plan and no organization to effectuate this project. Just what was meant by a demonstration school was never quite clear either. On the basis alone of personnel, materiel, and finances required, it appeared hopeless to attempt to start demonstration schools at this level in the foreseeable future. The inadequacy of transportation and communication to the interior militated against such a "hinterland" project. It was thus obvious that redirection was required, and, indeed, the Department of Public Instruction agreed that a fundamental education program along the lines of the UNESCO ideal should be attempted.

Furthermore, the total educational purpose of our mission seemed to have been inadequately conceived. At the bottom level of the educational plan, adult education, literacy, and primary schooling were to be provided in rural areas through fundamental education. University education was to be dealt with at the upper level. There was no provision for elementary education generally or for the intermediate school level. Just what this meant was forcefully illustrated by the problem confronting the science teachers at the university. In no case was it possible for them to conduct their courses at college level, due to the glaring inadequacy of the students' preparation. Few students at the college had a genuine understanding of the little that had been taught them by very willing teachers who were woefully deficient in knowledge. Elementary arithmetic was one of the outstanding deficiencies, and many students did not know the multiplication table. On the other hand, they had learned to perform mechanical operations in algebra and geometry. Laboratory experiments in physics, chemistry, and biology were quite alien to them, and these students were largely unfamiliar with the first principles in these fields. The result was that every science course had to be taught on a high school or, more accurately, a junior high school level. The students were willing and intelligent but, in addition to their lack of preparation, they were, for the most part, handicapped by malnutrition, chronic illness, and poor housing conditions.

The government, recognizing these problems, agreed to our plan for a science-teacher training program to be expanded beyond the ineffectual one at the university. This plan called for employment by the government of two science teachers for each of four government high schools. These teachers were to act as replacements for regular teachers in each school. They would conduct classes and assist the remaining Liberian science teachers to improve their instruction and expand their knowledge. The replaced teachers were to be sent to the university for training under the

UNESCO science teachers. When trainees returned to their teaching positions, others were to be sent for training. In this fashion, all science teachers at the high schools would receive, in rotation, further training both at the university and at the high schools.

We also recommended an increase in teachers' salaries in order to make the profession more attractive to competent people. The Secretary of Public Instruction subsequently took steps to implement this recommendation. Additional efforts were made to provide some medical supervision of the students at the university by the employment of a visiting nurse.

The university program was further handicapped by disagreements on objectives between the mission and the university administration. It was the desire of the latter to adhere to the development of a traditional broad liberal arts program. While we considered it desirable to have as wide a range of development as possible, our primary objective was the development of science. We felt that the emphasis on science as outlined in the agreement was entirely logical for this period in Liberian history. The emphasis on economic development consistent with the goal of national progress required that certain priorities be established. If, in addition to science teachers, skilled Liberian technicians, scientists, and engineers were ultimately to be produced in order to make that country self-reliant and resourceful in exploiting its natural resources without being dependent upon foreign skill, money, and enterprise, then it seemed to us that at least temporary stress should be placed upon a sound science program without necessarily excluding the liberal arts. Indeed, the liberal arts program, in quality of instruction and knowledge was of little worth anyway. If students could be properly trained as teachers and in other areas of the country's real needs, an important use would be made of them. Furthermore, Liberian students studying abroad as graduates or undergraduates could not hope to succeed as well as their foreign counterparts until better scientific training was available to them at home.

The university administration took the position that the only job the UNESCO science teachers had been asked to perform was teaching. Since the agreement stated that these men were to "develop and teach" their sciences, our position was that they were required to do more than just teach. An important part of UNESCO's technical assistance lies in its advisory role in the development of sound programs. If only teachers were wanted, then the university could hire them without technical assistance from UNESCO. Also, we felt that the mere teaching of science, unrelated to a meaningful program with definite objectives, would take too long to make its effects felt upon the country. On this issue, as in most others, the members of the mission, who were products of seven different educational systems, had no difficulty in achieving unity, and we felt justified in seeking similar unity with the university administration.

We feared that if our proposals for science training were not accepted,

that portion of our mission might have to be withdrawn. Withdrawing a mission, however, or even a part of a mission, creates a very sensitive diplomatic situation, and this sort of thing we wished to avoid. But we continued to press the issue, since we felt that influential persons in the government were in sympathy with the proposed program.

It is obviously impossible to detail all the issues of the controversy, which was still unresolved when I left Liberia. I have dwelt at some length on this phase of our mission's operations for several reasons. For one thing, half of our personnel were assigned to the university. For another, the science program was of essential importance to the government's over-all plans of economic development. But it is even more important to understand that the university dispute reflected not merely a problem in implementation. It involved the whole issue of the proper goal of higher education in an underdeveloped country. There is no intent here to disparage the viewpoint of a hard-working and conscientious university administration. The interests of Liberia were paramount in the considerations of both sides to the dispute. It was an honest disagreement between two educational viewpoints which had to be debated in an exchange of views that might have led to agreement.

Another important part of our mission was the development of a program of fundamental education. It has already been indicated that I early undertook to create interest and some organization for this work among cabinet ministers whose departments would be of direct assistance in implementing the program. While this program was designed to help the people help themselves, equipment and personnel would be needed in the fields of agriculture, health, literacy, and primary schooling. The government had no specific plans for initiating this program at any particular site, and it was necessary for us to gain some living knowledge of the country and its people before recommending a locale and a plan.

My wife and I took a week's trip by car through the interior of Liberia and into neighboring French Guinea. This trip was a hasty one, but we learned some things about the country to augment what we had previously learned of it from books. In addition, we had many lengthy discussions with educators, missionaries, and others who knew intimately the country and its people. We suggested that a pilot project be established in the small village of Dimeh, in the Klay district, about 20 miles from Monrovia. Dimeh had a population of only about 50 people, but it was the center of an area including many similar villages which we hoped to incorporate in the program.

A number of other reasons prompted our choice of this site for a pilot project. First, it was essential to maintain easy communication with Monrovia. If we were situated too far out in the bush we might easily be forgotten, but over the short road to Dimeh we should be able, most of the year, to bring in personnel, supplies and, by no means of least importance, government officials, whose continuing approval was always

essential. Second, Point 4 technicians had already done some agricultural work in the area and had won the interest of the people. The villagers had attended meetings, received instruction, and knew what assistance implied. Third, we had access not only to the local elders, but we had also hope of winning their confidence through a Liberian of Vai parentage and tribal origin, Bai Moore, who had been educated in the United States under the aegis of a Protestant Mission in Liberia, and who spoke fluently the languages of the area, which had a varied population, including Vai, De, and Gola with a scattering of Mandingo. Fourth, I had found the people in the villages friendly and receptive. While this attitude was no proof of ultimate cooperation on their part, Moore, who had a genuine sociological interest in the area, was held in high regard by the people and by the important men in the community, to most of whom he was related.

It was obviously impossible to supervise efficiently all aspects of the mission's program at the same time. I was needed constantly in Monrovia for, in all policy questions and controversial issues, the government would deal only with the chief of mission. Furthermore, frequent meetings of the entire mission personnel were required for routine administration and to develop a consistent policy in all aspects of our work. The head of mission was held responsible by both the Liberian Government and UNESCO for all aspects of the mission, but I was determined not to let the fundamental education program die a lingering death because of the demands of my other responsibilities or for lack of an expert in this field.

It was gratifying to find several members of the government developing real enthusiasm for fundamental education. I feared, however, that we were overreaching ourselves when it was decided that a party of government officials, including the Secretary of Interior, the Under-Secretary of Public Instruction, and myself would appear together at the District Commissioner's compound in Klay (Western Province), approximately 20 miles from Dimeh, to meet the paramount chiefs and other important members of the community and explain to them the new program that their government, with the assistance of UNESCO, was bringing. It was an amicable affair, including festivities, and I was pleased at seeing government officials take the step of making, in person, such commitments to their people. I was troubled, however, by the fact that the fundamental education specialist had not yet arrived, since we were on the threshold of the rainy season. Moreover, my conception of the proportions of the project site had been quite modest, including only Dimeh and its environs. But here were thousands of people from more distant parts of the Klay district being led to believe that they were to benefit from a wonderful new program in the not too distant future. I felt that it would take months to make a meaningful beginning.

At previous meetings with the people of Dimeh and neighboring villages, we had carefully explained the program to them and had sought their reactions and ideas. We had made no rash promises, but impressed

them with the fact that this was no "gravy train." We pointed out that the success of the project depended on the efforts of the people themselves, and that they would have to do the work. At our meetings, we asked the people what they really wanted out of the possibilities our program presented to them, including literacy for adults, literacy and primary schools for the children, improved agricultural techniques, marketing cooperatives, improved health and sanitation, medical treatment, development of handicrafts, *etc.* They were virtually unanimous in desiring education, particularly schooling and literacy for their children. As they put it, they wanted their children to "learn book" so that no one from Monrovia could put anything over on them.

Enthusiasm at these meetings was widespread, the more so since nothing was compulsory about the project. The people could take it or leave it. They proved their sincerity and enthusiasm in many ways. For example, after one of our visits, the people held a meeting and decided to surprise us. At our next visit we found they had cleared an area of bush and were beginning construction of a large palaver house to serve as a temporary center for our meetings and for whatever instruction would ultimately take place. We were very pleased, but this act gave us a feeling that we had set forces in motion which would be difficult to guide and utilize unless the fundamental education expert arrived soon.

Meanwhile, we shared in a common field experience of other anthropologists, in that many people came to us asking for medical assistance. In the beginning we employed whatever standard medicines we could secure – aspirin, antimalarials, iodine, vitamins, ointments. This experience stimulated our efforts to secure qualified medical help from the government as a first step. Chronically ill people such as these were could neither participate in, nor benefit from, the total program. This aid was difficult to secure but, with the assistance of the Health Mission attached to the Point 4 program under the supervision of the Liberian Bureau of Public Health and Sanitation, we in time obtained medical supplies, a doctor, a nurse, and a laboratory technician for regular weekly visits. The early clinics were conducted from the back of a pick-up truck, but they were soon transferred to one of the village huts.

Weather and roads permitting, we went out to the villages every afternoon when it was possible for me to leave the work in Monrovia. My wife and I did what little we could in the short time available, and, as a result, the residents of Dimeh themselves made various small improvements. The village was cleaned, mud puddles were eliminated, some women had shelves built for their pots and other utensils, some men were induced to dig pit latrines, and to make compost heaps of their garbage. It was a pitifully small effort, but it, too, kept interest alive. The really important thing was that the people saw us often and gained confidence in us and our promises of assistance.

As an anthropologist, I was struck by the fact that a lack of previous

field experience in Africa, or even of an extensive knowledge of native Liberian cultures, did not seem to matter much, perhaps because we were fortunate in having the assistance of Moore, our Vai friend. Moore, too, recognized that the people knew what they wanted, that they were aware of potential benefits from Western technology and knowledge, and that cultural differences were not a profound barrier. Our standard approach was to work through local persons who had the respect and confidence of the people. Earlier, we had expected to have to work through paramount chiefs and the bush schools (the Poro and the Sande), but we found that a direct approach to problems of importance was better.

Our fundamental education expert finally arrived in October 1951, six months after the mission had begun. He had done educational work in the northern provinces of Nigeria for 22 years. He spoke Hausa and Fulani, and was sensitive to the problems of the people of Africa. I was happy to turn the project over to his wisdom and experience. This expert settled in Dimeh. Under his direction, village schools were established with Liberian teachers trained by him. Acting under his supervision, these teachers instructed the children in their local languages. Normally, teaching is done in English and is poorly understood, if at all, by the tribal people. The school buildings were open palaver houses with thatched roofs, and had mud walls two feet high. Some existent structures were used; others were newly built by the men of the village. In the beginning, before any school supplies were available, the floors were covered with a layer of fine sand, and the children sat on mats in orderly rows, performing their lessons with pointed sticks on the sandy floor. The teacher used charts prepared under the direction of the fundamental education expert. Instruction, in addition to the three R's, was given in handicrafts, personal hygiene, singing of songs, English, and new types of games. Each child was responsible for a plot in the school garden. There were also night classes in literacy for the adults.

The clinic was efficiently organized and expanded. Case histories and other records were carefully maintained. Different types of clinics—including prenatal, gynecological, and venereal—were held. By the end of my stay, over 1,200 registered patients from more than 80 villages were attending this clinic. According to later information, by June 1953 the number of registered patients was well over 3,000, from more than 120 villages. Some people walked distances so great that it took them as long as eight hours to reach the clinic.

Ultimately, the fundamental education specialist began construction of buildings for a fundamental education and administration center at Klay. A 10-bed clinic and hospital was built and equipped in a village about one mile from Dimeh. It was staffed with a resident nurse, and with a visiting nurse who went regularly to all the villages in the area, while a doctor was assigned by the government to make more frequent visits from Monrovia.

A significant part of this program was the training of Liberian personnel in the methods and techniques of fundamental education. One Liberian was sent abroad to participate directly in similar programs that were being conducted by other national and international agencies. Moore, who originally helped us to establish the program at Dimeh, acted as assistant to the fundamental education expert, and worked directly with him at every step of planning and executing all phases of the project. Finally, all the teachers in the primary schools that were established under the program received instruction and demonstrations from the fundamental education expert during his visits to the schools and at periodic meetings of all the teachers. The important intermediary in all this work was Moore.

Concerning the work in psychology, no culture-free tests were devised, but standard intelligence and performance tests were adapted to the Liberian situation and were given to students in about a dozen schools, mainly in the coastal regions. The results of these tests confirmed what we had originally assumed: that a range of talent waited to be brought to fruition with proper educational opportunity. These tests were to be used by the Department of Public Instruction in its grants of scholarships and fellowships to worthy students for study at the University of Liberia and abroad. They were also to be used in connection with a proposed program of vocational guidance. The psychologist was originally employed for a term of three months, during which he was supposed to function in connection with nonexistent demonstration schools. His contract was extended to 12 months. In addition to his work on the tests, he taught psychology courses which involved the training of students in the preparation and administration of tests,

The vocational education specialist experienced considerable difficulty in getting his project under way. According to agreement he was to teach in a shop, but no such shop existed. With funds from the Liberian Government and from UNESCO we ordered a large quantity of equipment which was very slow in arriving. Meanwhile, this specialist taught French at the university, collaborated with TCA in supervising a course in clerical work, and made a survey of various skills existing and needed in Monrovia. The survey showed that virtually all the skills needed in the city were difficult to find, and that those existing were at a very low level of efficiency. We felt that vocational education should be introduced into the Liberian school system, and asked the expert to prepare proposals. They had not been submitted when I left.

Before I left Liberia, a supplemental agreement was concluded between UNESCO and the Liberian Government which expanded the program, but omitted psychology. The program was to be "managed by Joint Directors, one appointed by the President of Liberia, and the other by UNESCO." The major fields of operation were designated as Fundamental Education, Rural Elementary Education, Teacher Training, Technical Education, and University and Science Education. The fundamental education project

was to be provided with six additional experts in the fields of literacy, women's education, health education, rural elementary teacher training, agricultural extension or home economics, rural arts and cooperatives. Other details of the agreement covered much more extensive financing by UNESCO and Liberia, and the provision of additional fellowships and study grants by UNESCO in all major fields of operation. The new contract was to continue for a period of five years, with the option of an additional five.

The results of the program, as of June 1952, may be summarized in the following way:

(1) *University science.* Few of our recommendations had been carried out, and the dispute regarding objectives and curriculum continued. UNESCO teachers with very high qualifications were obliged to give their courses at a low high-school level. However, laboratory equipment purchased with funds from the Liberian Government and UNESCO had been installed and more was on order. Contact had been established with foreign countries in the effort to recruit high-school science teachers, who were later employed by the government.

(2.) *Psychology.* Standard tests were prepared and administered to students, and psychology courses were taught at the university until February 1952, when the psychologist's contract expired. The results of the tests were turned over to the Department of Public Instruction. I do not know if they have been used or if they ever will be.

(3.) *Vocational education.* Substantially nothing had been accomplished.

(4.) *Fundamental education.* This program may be considered the most positive achievement of all our efforts. Unofficial reports that I have received indicate that we were successful in laying a firm foundation for the continuation of this type of work by the government, and that it may be a lasting contribution of UNESCO's assistance to Liberia. At least 20 primary schools have apparently been established throughout the area of this project. A hospital, with permanent clinic, has been built, equipped, and staffed, and regular medical attention is available. Just how far a health education program has been developed I do not know, although this was important in our plans. We had ordered health films and film strips, and similar material was available for this work from the United States Information Service. These materials are now being used in the program. Important also is the fact that Liberians are being trained to take over, to continue, and to develop further such projects as were introduced. If some gratification can be derived from the work of our mission, it must come largely from our achievements in fundamental education.

There are several factors which may explain the successful growth and development of this program. First, it met the genuine needs of the people it touched, so that they responded with whole-hearted cooperation. Second, a sound basis was laid in government by inculcating among offi-

cials the feeling that the progress of Liberia depended in great part upon this kind of development of its human resources. Third, there were the devoted and unselfish efforts of the fundamental education specialist whose long experience in Africa had given him profound knowledge of his job and of the needs and desires of Africans. Fourth, Bai Moore's knowledge of the local situation and large influence among the people helped us immeasurably. Fifth, the work could be done directly with the people it was intended to benefit, so that we did not encounter those problems which dissipated our time and energy at the university.

(5.) *Acceptance by the Liberian Government and people.* No mission can hope to survive or succeed without winning both popular and official confidence. This much we achieved in spite of some difficult and estremely sensitive situations. The fact that we succeeded in this respect was in no small measure a tribute to the kind of men of which most of the mission was composed.

I have included in this discussion material which should indicate the conditions and problems that confronted our mission and, in particular, those facing the head of the mission who, originally, was assigned the unenviable task of carrying on dual functions. In such circumstances I decided that I should consider myself fortunate if I could have the opportunity of performing one of those functions with a reasonable chance of success. As I saw it, there was no alternative but to drop the role of anthropologist and assume in full the basic responsibilities as head of mission.

Capable and effective administration is not a monopoly of anthropologists. It is a quality possessed by people in many fields, though anthropological training may sharpen this skill. Of what value, if any, was my anthropological training to my administrative function in this program? This question is difficult to answer, because I cannot separate the anthropological from the more strictly personal component. Those qualities which I thought might be attributed to my anthropological background I saw in equal measure in a Danish mathematician, a New Zealand biologist, an Indian chemist, and a British educator, none of whom had ever benefited from anthropological training. All had a sensitive perception of the cultural and psychological situation in which we were functioning. They had a deep feeling for the people whom they had come to assist, and a genuine human understanding of their problems. These attributes explain why they came so close to their students and gave them so much of their time. The fundamental education specialist likewise devoted himself to the people among whom he lived. All these men projected themselves into the lives of the people they were working among, and the people rewarded them in a profoundly human way with friendship and confidence. There are certainly many non-anthropologists who have acute culturalsensitivities and perceptions, just as there are undoubtedly anthropologists who lack these qualities.

The anthropologist, in purely professional capacity, would be of most assistance to action programs through preparatory research and through continuing research as a team member on a long-term basis. Another important contribution he could make would be to study the processes and the effects of such action programs. This suggestion means that if an anthropological contribution is expected, the anthropologist must be free to do field work relatively unencumbered by other major duties.

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DIVISION OF MYCOLOGY

THE SIGNIFICANCE OF SAPROPHYTISM IN THE EPIDEMIOLOGY OF THE MYCOSES*

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In 1951, I reported some studies of the occurrence in soil of fungi which cause human diseases.[1]

Since the first isolations of Histoplasma,[2] Allescheria,[3] and Cryptococcus,[4] from soil, we have continued at the National Institutes of Health to examine collections of soil and other materials from various types of environments either for the information to be gained about the distribution and habitats of pathogenic fungi or to determine, if possible, the source of infection in proved or suspected cases of histoplasmosis. Only a few of the data have been reported, and it is desirable to summarize the information we have obtained to date about the occurrence in nature of some of the fungi which cause mycoses.

Materials and Methods

Specimens have been collected in 25 × 150 mm. cotton-stoppered glass tubes by scooping up superficial layers of soil directly into the mouth of the tube. The specimens have consisted of garden and lawn soil, soil or dust from basements of residences, chaff and litter from barns and chicken houses, soil from protected areas under eaves and edges of farm buildings, and bird and animal excreta in various stages of decomposition.

Specimens have been brought to the laboratory and usually processed immediately or stored at 2 to 5° C. and processed the next day. The method of examination has been essentially that of Stewart and Meyer,[5] who first isolated Coccidioides from soil. The use of physiological saline instead of concentrated saline, and of mice instead of guinea pigs, are the principal modifications. We have adopted other minor modifications from time to time, and the following schedule is currently in use.

Transfer 10 to 15 ml. of soil to a 25 × 150 mm. tube, add 0.8 per cent NaCl solution to nearly fill the tube, insert a rubber stopper, shake the mixture vigorously for 5 seconds, allow the suspension to sediment for 15 minutes, pipette 4 ml. of supernatant from the top of the column

*This paper, illustrated with lantern slides was presented at a meeting of the Division on November 26, 1954.

into a conical glass, add 1 ml. of antibiotic solution (2 mg. streptomycin and 5 mg. penicillin per 1 ml. water), and inject 1 ml. of this mixture intraperitoneally into each of 4 Swiss, white, general-purpose mice. Hold the mice 4 weeks, then kill them and make cultures on Sabouraud's agar from spleen and liver. Inoculate two slants from each organ by smearing a piece of tissue (100 to 300 cu. mm.) over the surface of each slant. It is usually possible to observe growth from infected mice in 10 days but cultures must be held one month before they are discarded as negative. Thus there is an interval of approximately two months between collection of the specimen and the recording of results.

In most cases we have processed a second portion of the specimen 1 to 2 months after collection in order to confirm the first examination or to test the effect of storage at room temperature upon survival or multiplication of the fungus in the collected specimen.

Results

Since 1946, working intermittently on this study, we have processed 1751 specimens. TABLE 1 presents a summary of the results of these examinations.

Isolation of Histoplasma. An examination of TABLE 1 discloses at once an uneven distribution of the 104 specimens positive for *Histoplasma capsulatum* among the 1751 specimens studied. There is an obvious association with the presence of chickens, since 73 of the 104 were litter from chicken houses or soil adjacent to or under the edge of chicken houses. These figures include the first two isolations reported from soil,[2] and these specimens were taken from mounds at the mouth of rat burrows under the edge of a chicken house.

The association with chickens in this series is even closer than appears from a cursory examination of the table. The dog pen from which, as is indicated, two isolations were made was adjacent to an old chicken house which may have been a more significant factor than the presence of dogs. No dogs with recognized histoplasmosis were found on this farm, although no dogs were obtained for sacrifice and laboratory study. The 18 isolations from the dirt floor of a meat house reflect only the situation in one individual meat house from which, after the first isolation was made, specimens were collected repeatedly. Therefore it must be considered, for the present, an isolated and unusual instance. Finally, 5 of the 11 positive samples from miscellaneous farm buildings were collected near or under a rabbit hutch from which multiple collections were made after it was found to be a productive source of Histoplasma. Chickens had free access to this area.

Another item of interest not immediately apparent in the table is the demonstrated persistence of Histoplasma in soil. Histoplasma was isolated from two specimens of litter which had been taken from a chicken

TABLE 1

ISOLATION OF PATHOGENIC FUNGI

(Except as noted, collections were taken within a radius of 75 miles of Washington D. C.)

	Collections	*Histoplasma isolated*	*% pos.*	*Cryptococcus isolated*	*Allescheria isolated*	*Sporotrichum isolated*
Inside chicken house	62	22	35	--	--	--
Adjacent to chicken house	228	48	21	--	2	--
Piled litter from hen house[1]	18	2	11	--	--	--
In chicken yard	25	1	4	--	--	--
In meat house	76	18[2]	23	--	--	--
In dog pen	9	2	22	--	--	--
Miscellaneous farm buildings	532	11	2	12[3]	2	2
School yard	12	--	--	--	--	--
Residence, yard, under porch	168	--	--	--	--	--
Residence, in basement	156	--	--	--	--	--
Exposed attic, old house	11	--	--	8[4]	--	--
Garden	25	--	--	--	--	--
Virginia and W. Va. caves	14	--	--	--	--	--
Southern Georgia[5]	361	--	--	--	--	--
Near Albany, N. Y.[5]	40	--	--	--	--	--
Forest soil, Madison, Wis.	14	--	--	--	--	--
Totals	*1751*	*104*		*20*	*4*	*2*

[1]Litter removed from chicken house six weeks previously and left in small piles in field.
[2]All 18 positive collections from one meat house.
[3]Four positive collections from near granary with possible contamination by pigeon droppings.
[4]All 8 positive collections from weathered pigeon droppings, attic of an old house.
[5]Most of the collections from Georgia and New York were from chicken houses. Because all collections from Georgia and New York were negative, they are segregated to avoid dilution of data.

house and deposited in small piles on an exposed hillside about six weeks before.* In another instance Histoplasma was isolated from bluegrass sod at a site where a chicken house had been torn down three years before. The patient in this case had assisted in the demolition of the building and had used some of the chicken manure for fertilizer.**

The spotty distribution of Histoplasma on a given farm premise was apparent in almost every instance where repetitive sampling was done. Once Histoplasma was located at a given spot, some samples in subsequent collections were usually consistently productive, while samples taken from other nearby buildings were negative. On one large tobacco farm, 50 samples were taken from in or near the numerous barns and outbuildings on the farm. Only four were positive, and these samples were from a small chicken house. In three subsequent samplings of this chicken house, 25 to 70 per cent of samples in a given collection yielded Histoplasma.†

Isolation of Cryptococcus neoformans. This organism was isolated from 20 samples in this series. Many of these samples bore a curious and possibly significant relationship to pigeons. Eight specimens were essentially old weathered pigeon droppings taken from the attic of an old house from which the dormers and gables had long been removed and in which pigeons had been nesting and roosting. One of the other 12 was from debris from a pigeons nest‡ and 4 were from a granary with probable contamination by pigeons. This source recalls some of the epidemics of pneumonitis presumed to be histoplasmosis in which pigeon manure was prominently present in the environment of the workmen involved. If *C. neoformans* is found frequently in old accumulations of pigeon droppings, cryptococcosis will have to be considered in the differential diagnosis of future similar epidemics of pneumonitis.

Isolations of Sporotrichum and Allescheria. The two isolates of Sporotrichum listed in TABLE 1 were typical of *S. schenckii*, and they provide additional evidence of the occurrence of this fungus in soil. No conclusions could be drawn from this brief experience concerning a predilection of the fungus for any particular substrate.

Allescheria boydii has been isolated previously from soil,[3,8] and this report adds three additional isolations. In addition to these, further evidence concerning the occurrence and geographic distribution of *A. boydii* has come to my attention by the receipt of two collections

*These specimens were obtained through the cooperation of Doctor Walter Pacosa, Children's Hospital, whose patient had presumably been infected during performance of this task.

**The environmental studies in this case were made possible through the cooperation of Doctor Norman McCullough, Clinical Center, National Institutes of Health.

†This was the presumptive source of infection of a patient of Doctor Sol Katz, District of Columbia General Hospital, through whose courtesy I was able to carry on this investigation.

‡Submitted by Charlotte Campbell, United States Army Medical School, to whom I am indebted for the opportunity to study this specimen.

of this fungus for identification. One was received from Doctor Roy Cain, with the statement that the fungus is one of the common soil fungi in the vicinity of Toronto, Canada. The second collection was from Doctor Marie B. Morrow, who isolated the fungus from air-borne spores in the course of aerobiological studies in Texas.

In addition to the data shown in the table, other fungi of interest to the mycologist have been isolated. *Phialophora verrucosa* was isolated once from decaying wood taken from siding from an old barn. *Nocardia asteroides* was isolated once. Certain nonpathogenic fungi which can survive mouse passage, such as *Microascus* spp. and *Aspergillus clavatus* have been repeatedly isolated, and these and other isolations have been reported.[3]

Discussion

The primary purpose of the studies reported has been the detection of Histoplasma in soil, and the techniques used have been designed to achieve this purpose. The methods used may well be more efficient in the isolation of Histoplasma than of other pathogenic fungi. Their efficiency is related to the susceptibility of the mouse to infection by a single spore of Histoplasma.[6] The systemic (although benign) nature of the ensuing disease makes isolation of Histoplasma from the experimental mouse easier than isolation of a fungus such as Blastomyces which, with a similar minimal infecting dose and within the same time interval may produce only an inconspicuous omental abscess. The extreme sensitivity of this technique and its resultant vulnerability to error have recently been discussed in another paper.[7]

Ajello and Zeidberg first called attention to the association between the saprophytic occurrence of Histoplasma and the presence of chickens, and they have discussed the implications of this association.[8,9,10] It is the general concensus that chickens are not hosts of Histoplasma, and that the relationship is merely a saprophytic association with soil enriched by chicken excreta. This conclusion is supported by our own experience in a search for animal hosts of Histoplasma. In the course of autopsies of 4664 animals, 21 chickens were examined.[11] The chickens were dead when submitted for examination, or were ill when obtained from flocks showing evidence of a similar disease. Many of these birds had greatly enlarged spleens and livers which grossly suggested histoplasmosis, but appropriate studies* established the diagnosis of avian tuberculosis or lymphomatosis. Histoplasmosis was not observed in any chickens.

The familiar concept of an infectious disease is that of an illness which follows either the activation of an erstwhile latent endogenous agent, or the transmission of an infectious agent from an infected man

*Doctor Robert Haberman interpreted and reported the pathology in these chickens.

or animal to a susceptible host. Transmission may be consummated by direct contact or by exposure to pathological excreta or exudates. It may require the intervention of an intermediate host or vector, as in insect transmission of malaria. Or, transmission may be more remotely related to an animal or human host when it involves only exposure to soil contaminated with embryonated eggs of a parasite, or to soil contaminated with Clostridium spores from an infested animal host. I am aware that there are exceptions but, in general, the viral and bacterial diseases appear to require an essentially unbroken (although sometimes tenuous) chain of human or animal hosts for survival.

In the subcutaneous and systemic mycoses, no such necessary relationship to an antecedent human or animal host has been demonstrated. The epidemiology of most of the deep mycoses is that of a group of diseases caused by fungi which are primarily saprophytes capable of living and reproducing indefinitely in soil, without any necessary relationship to a living animal host.

The concept that pathogenic fungi can be isolated from substrates in nature is not new. The story of cryptococcosis illustrates the early demonstration of this relationship. In an era when an infectious agent of cancer was being sought and bacteriologists and pathologists were testing the pathogenicity of many microorganisms, Sanfelice, in 1895, isolated a yeastlike fungus from fruit juices, found that it produced tumorlike lesions in experimentally infected animals and named it *Saccharomyces neoformans*.[12,13] This organism proved to be the same yeastlike fungus which Busse had isolated from a human patient.[14] Benham's later studies, showing the common etiology of meningeal and systemic cryptococcosis, and the similarity of the fungus to avirulent strains found on body surfaces, elucidated relationships between certain yeastlike pathogens which had been a source of considerable confusion in earlier medical literature.[15] More recently, *C. neoformans* has been found to be present not uncommonly in soil and pigeon droppings without apparent relationship to either fruit or human or animal disease.[4] If further experience substantiates the association of *C. neoformans* with excreta of pigeons or other birds, as suggested in the studies reported here, it should be sought as an etiological agent of disease in pigeons but, at present, there is no reason to suspect a host-parasite relationship. The decomposed and weathered condition of the specimens examined in the present study suggests rather a purely saprophytic relationship.

Without reviewing again all the many reported isolations of pathogenic fungi from soil, a few instances of historic or other special significance should be mentioned. The isolation of Coccidioides from soil by Stewart and Meyer[5] is notable because it established the usefulness, in such a search, of the technique of animal inoculation with a soil suspension. This technique, with various modifications, has been used by many

investigators to amass a considerable amount of information about the saprophytic occurrence of pathogens. The demonstration of Coccidioides in soil supported the pattern indicated by epidemiological studies of a pulmonary disease acquired by inhalation of air-borne spores of the saprophytic growth phase of the fungus.[16,17] This pattern reappears in other pulmonary or systemic mycoses caused by dimorphic fungi which occur in animal tissues as fragile and vulnerable cells, and in nature as resistant, numerous, and easily disseminated spores.

In the subcutaneous mycoses represented by sporotrichosis, the importance of a saprophytic source of infection is most dramatically illustrated in the epidemic of this mycosis in gold mines of the Witwatersrand near Johannesburg, South Africa.[18] Three thousand cases of sporotrichosis were reported among the men working in these mines. *Sporotrichum schenckii* was isolated easily and repeatedly from the timbers in the mines, and control of the epidemic was achieved not by medical measures but by sanitary measures, including the application of fungicides to the mine timbers.

In the superficial mycoses, the dermatophytoses, the relationship of human infection to a saprophytic reservoir of the dermatophytes is probably different. The ubiquity on the skin and nails of fungi causing "athlete's foot" appears to minimize the epidemiologic importance of newly acquired infections of this type. At least it greatly obscures the picture. Some of the dermatophytes do exist in a saprophytic reservoir, however. The most important instance of this finding so far recognized is the occurrence in soil of *Microsporum gypseum*.[10,19,20,21] Human infections caused by this fungus are sporadic in occurrence and distribution, and cannot be explained by transmission from another human infection and, only very rarely, from an animal. The demonstration that *M. gypseum* is frequently present in soil and can be easily isolated by "baiting" soil with hair,[22] suggests an immediate source of human infection. It does not answer the obvious question of whether this keratolytic fungus is present in soil because of the presence of an as yet undetected infected animal host.

One item of new data on the saprophytic occurrence of pathogens relates to mycetomas. It has always appeared obvious that there must be a relationship between the traumatic subcutaneous implantation of fungus spores and development of mycetoma. I have already mentioned the isolations of *Allescheria boydii* from Canada and from Texas, the state in which this fungus was first isolated. These observations further establish the saprophytic occurrence of this important agent of mycetoma which, for nearly 30 years, was known only from human lesions, and they emphasize the fallacy of assuming that fungi causing mycetoma are limited to a tropical distribution.

This temperate-zone distribution of fungi whose pathogenic activities are mainly apparent in tropical and subtropical zones is illustrated

also by *Phialophora verrucosa* (Cadophora americana).[23] It is by no means a new concept that the traumatic mycoses are more common in tropical areas probably because there is more opportunity for implantation of spores, but it bears repeating. Both mycetoma and chromoblastomycosis are seen most often in persons exposed to trauma by working in the fields without protection of shoes. The saprophytic occurrence of the second and closely related agent of chromoblastomycosis, *Phialophora pedrosoi*, has not been conclusively demonstrated, but it may be assumed. One might ask why, if the etiologic agents are present, these diseases are not seen in children who play barefoot in temperate climates, but it must be noted that these mycetomas are of greatest frequency in the adult, rather than in the child population, even in the tropics.

The conditions under which a pathogenic fungus can initiate an infection are not fully known, nor should it be assumed that they are the same for different fungi. They relate to viability of the fungus, to exposure of the host to an infecting dose of suitable size, to a suitable route of infection (*e.g.*, by ingestion, inhalation, or traumatic subcutaneous implantation), to the immunologic status of the recipient host and, most important since it is primary to other conditions, to the presence of the etiologic fungus in the environment in a form suitable for dissemination.

With respect to viability, Histoplasma is a good example of a fungus which in its parasitic growth phase rapidly loses its viability in culture and in suspensions prepared for experimental use. Its saprophytic growth phase, on the contrary, produces conidia and macroconidia which are resistant to deleterious environmental influences. Evidence supporting the concept that these conidia are important etiologic agents in certain epidemics of pneumonitis is rapidly accumulating.[24,25] This contrast between viability of parasitic and saprophytic growth phases is found also in *Coccidioides immitis*. In such mycoses as histoplasmosis and coccidioidomycosis, exposure of either man or experimental animals to a large infecting dose of spores is closely and normally related to the development of a progressive systemic disease. On the contrary, a few spores may well be more effective in initiating a subcutaneous fungus infection. Attempts to produce mycetomas experimentally by injection of spores or insertion upon or with a foreign body have suggested that a large infecting dose of these fungi may induce a severe host reaction which results in effective drainage of the penetrating wound.

The condition prerequisite to infection, the presence in man's environment of viable, easily disseminated elements of the pathogenic fungus, is best satisfied by the saprophytic growth of the fungus. This growth provides the more resistant form of the dimorphic fungus, and these conidia may be disseminated in a veritable cloud of air-borne spores,

or they may be attached to a penetrating thorn in numbers small enough to be retained in subcutaneous tissues until they have established a firm parasitic relationship. Finally, laboratory studies have demonstrated conclusively an actual saprophytic reservoir of many pathogenic fungi compatible with the epidemiology of the mycoses they cause.

Summary

Results of the examination of 1751 specimens of soil, dust, litter, and animal and bird excreta are summarized. Pathogenic fungi were sought by injecting the supernatant from a suspension intraperitoneally into mice. *Histoplasma capsulatum* was isolated from 104 specimens, *Cryptococcus neoformans* from 20, *Allescheria boydii* from 4, and *Sporotrichum schenckii* from 2.

The implications of these observations for the epidemiology of the mycoses are discussed.

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CONFERENCE HELD

SECTION OF BIOLOGY

THE REGULATION OF HUNGER AND APPETITE

Friday, November 19 and
Saturday, November 20, 1954

Conference Chairman: Franklin Hollander
Gastroenterology Research Lab.
The Mount Sinai Hospital, New York, N. Y.

FRIDAY, NOVEMBER 19

REGULATORY MECHANISMS

Session Chairman: Albert J. Stunkard, Depts. of Medicine & Psychiatry
New York Hospital, New York, N. Y.

9:30 A.M. –
Opening Remarks – Franklin Hollander, Gastroenterology Research Lab., The Mount Sinai Hospital, New York, N. Y.

"The Role of the Gastrointestinal Tract" – J. P. Quigley, Division of Physiology, University of Tennessee, Memphis, Tenn.

"The Glucostatic Mechanism of Regulation" – Jean Mayer, Dept. of Nutrition, Harvard School of Public Health, Boston, Mass.

"Neural Regulation of Food Intake" – John R. Brobeck, Dept. of Physiology, School of Medicine, University of Pennsylvania, Philadelphia, Pa.

"The Time Factor in Adjustment of Food Intake to Varied Caloric Requirement" – Henry D. Janowitz, Gastroenterology Research Lab., The Mount Sinai Hospital, New York, N. Y.

DISORDERS OF REGULATION

Session Chairman: Neal E. Miller
Dept. of Psychology, Institute of Human Relations,
Yale University, New Haven, Conn.

2:00 P.M. –
"Dietary Self-Selection in Experimental Animals" – Curt P. Richter, Dept. of Psycho-Biology, Johns Hopkins Hospital, Baltimore, Md.

"Role of the Emotions in Hunger and Appetite" – Hilde Bruch, Dept. of Psychiatry, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Integration of Current Views on the Regulation of Hunger and Appetite"– Morton I. Grossman, Physiology Division, Medical Nutrition Lab., Fitzsimmons Army Hospital, Denver, Colo.

"Discussion and Question Period" – Moderator – Alfred W. Pennington, Medical Division, E. I. du Pont de Nemours, Newark, Del.

SATURDAY, NOVEMBER 20

CLINICAL ASPECTS

Session Chairman: M. L. Tainter
Sterling-Winthrop Research Institute, Rensselaer, N. Y.

9:30 A.M. –
"Food Preferences in Man: Special Cravings and Aversions" – Isidore Snapper, Beth-El Hospital, Brooklyn, N. Y.

"Study of Hunger and Appetite in a Young Man with Esophageal Obstruction and Jejunostomy" – Franklin Hollander, Gastroenterology Research Lab., The Mount Sinai Hospital, New York, N. Y.

"Clinically Useful Appetite Depressants" – Stanley C. Harris, Dental School, Northwestern University, Chicago, Ill.

"The Action of Various Deleterious Compounds on Voluntary Intake" – Samuel Lepkovsky, Dept. of Poultry Husbandry, College of Agriculture, University of California, Berkeley, Calif.

Discussion and Question Period – Moderator – M. L. Tainter, Sterling-Winthrop Research Institute, Rensselaer, N. Y.

NEW MEMBERS

Elected November 18, 1954

SUSTAINING MEMBERSHIP

Bersworth, Frederick C., D.Sc., Organic Chemistry. President, Versenes Inc., Framingham, Mass.

Furniss, William E., M.D., Obstetrics and Gynecology. Chief, Obstetrics and Gynecology, Bristol Hospital, Bristol, Conn.

Roscoe, Andrew S., Economics. President, South Brooklyn Savings and Loan Association, Brooklyn, N.Y.

Underwood, Lee C., M.D., Cardiology. Private Practice, Canton, Ohio.

Van Zyl, Gerrit, Ph.D., Organic Chemistry. Head, Department, Chemistry, Hope College, Holland, Mich.

Vonder Heide, Elmore C., M.D., Hematology. Associate Director, Department, Clinical Investigation, Parke, Davis and Company, Detroit, Mich.

Warwick, Harry A., M.D., Pharmacology. Attending in Obstetrics and Gynecology, Beth-El Hospital, New York, N.Y.

Weinstein, Rayford Lee, M.D., Biology. Weinstein Clinic, Fairmont, S.C.

White, Philip Taylor, M.D., Diseases of the Nervous System. Assistant Professor, Neurology, Indiana University, Indianapolis, Ind.

ACTIVE MEMBERSHIP

Albertson, Edmond C., M.D., Internal Medicine. Private Practice, San Francisco, Calif.

Balinsky, Benjamin, Ph.D., Psychology. Associate Professor, Psychology, City College of New York, New York, N.Y.

Baron, Howard Charles, M.D., Surgery. Instructor, Surgery, New York University, Bellevue Medical Center, New York, N.Y.

Benda, Clemens E., M.D., Neuropsychiatry. Director, Research and Clinical Psychiatry, W. E. Fernald State School, Waverley, Mass.

Boyarsky, Saul, M.D., Renal Physiology. United States Public Health Service, Post Doctorate Fellow, Department, Physiology, New York University Bellevue Medical Center, New York, N.Y.

Braunwald, Eugene, M.D., Physiology. Postdoctoral Research Fellow, United States Public Health Service, New York, N.Y.

Bressler, Rubin, B.S., Biochemistry. Research Assistant, Beth Israel Hospital, Boston, Mass.

Chuker, Melvin S., M.D., Hematology. Associate Professor, Clinical Pathology, Miami Beach, Fla.

Crews, Marjorie Anne, M.D., Industrial Practice. Physician, Jackson Heights, N.Y.

Davidson, Arnold B., M.A., Pharmacology. Biological Research Assistant, U.S. Army, Brooklyn, N.Y.

De Jesus, Jose A., M.D., Internal Medicine. Associate Professor, Medicine, University of Puerto Rico, San Juan, Puerto Rico.

Dwyer, Thomas L., M.D., Medicine. Private Practice, Mexico, Mo.

Eron, Leonard D., Ph.D., Psychology. Assistant Professor, Psychiatry, Yale University, New Haven, Conn.

Farr, James N., Ph.D., Psychology. Assistant Professor, New York University, New York, N.Y.

Fitz, Thomas Edmunds, M.D., Internal Medicine. Internist, Stevens Clinic Hospital, Welch, W. Va.

Fleming, David G., Ph.D., Physiology. Assistant Professor, University of Kansas, Lawrence, Kans.

Fletcher, Celine Rosner, M.D., Hematology. Clinical Assistant, Memorial Hospital, New York, N.Y.

Freed, Leon C., M.D., Internal Medicine. Instructor in Medicine, University of Oklahoma, Oklahoma City, Okla.

Friedman, A. I., M.D., Gastroenterology. Private Practice, Hackensack, N.J.

Frye, Robert R., M.D., Medicine. Private Practice, Mannington, West Va.

Furst, Joseph Brown, M.D., Psychiatry. Private Practice, New York, N.Y.
Galletti, Giovanni, M.D., Surgery. Resident Physician, Buffalo State Hospital, Buffalo, N.Y.
Gladstone, Arthur, M.D., Gastro-intestinal Tract. Associate Professor, Clinical Surgery, University of Vermont, Burlington, Vt.
Hallinger, Leonard Norman, M.D., Medicine. Private Practice, New Rochelle, N.Y.
Ingham, G. K., M.D., Medicine. McGregor Clinic, Hamilton, Ont., Can.
Isaacs, Julien H., M.D., Medicine. Private Practice, Beverly Hills, Calif.
Jackson, Marvin Alexander, M.D., Pathology. Teaching Assistant in Pathology, University Hospital, Ann Arbor, Mich.
Jackson, Thomas Fred, M.D., Radiology. Associate Radiologist, Southern Baptist Hospital, New Orleans, La.
Jarvik, Norman E., M.D., Medicine. Private Practice, Glenbrook, Conn.
Jensen, Uffe T., M.D., Medicine. Staff, City Hospital, Akron, Ohio.
Johnson, Gordon Neil, M.D., Surgery. Chief, Staff, Aroostook General Hospital, Houlton, Me.
Jones, Richard E., B.E.E., Medical Instrumentation Development. Head, Section on Engineering, Mayo Clinic, Rochester, Minn.
Joseph, Morris, M.D., Surgery. General Surgery, Passaic, N.J.
Kalman, Sumner M., M.D., Endocrinology. Instructor, Department of Pharmacology, Stanford University, San Francisco, Calif.
Pick, Joseph C., Geology. Layman, Brooklyn, N.Y.
Rodriguez-Forteza, Felix A., M.D., Biology, Psychology. Surgeon, Puerto Rico State Insurance Fund; Private Practice, Hato Rey, Puerto Rico.
Sacktor, Bertram, Ph.D., Biology. Entomologist, Medical Laboratories, Army Chemical Center, Md.
Salkind Victor A., Engineering. Scientific Attache, Embassy of Israel, Washington, D.C.
Sencer, Walter, M.D., Medicine. Assistant Attending Neurologist, Mt. Sinai Hospital, New York, N.Y.
Siegel, Charles D., B.A., Physiology. Research Assistant, Doctor A. S. Gordon, Forest Hills, N.Y.
Taylor, Reginald Murray, M.D., Neurophysiology. Associate Research Scientist, New York Psychiatric Institute, New York, N.Y.
Thron, Daniel M., M.D., Medicine. Private Practice, New York, N.Y.
Toogood, John Howard, M.D., Allergy. Instructor, Medicine, University of Western Ontario, London, Ont. Can.
Udinsky, H. J., M.D., Dermatology. Private Practice, Passaic, N.J.
Ury, Oskar, M.D., Medicine. Private Practice, San Francisco, Calif.
Vallecillo, Fernando, Jr., M.D., Medicine. Private Practice, Hato Rey, Puerto Rico.
Vallis, Charles P., M.D., Tissue Transplants. Private Practice, Lynn, Mass.
Van Gelder, D. W., M.D., Pediatrics. Assistant Professor, Tulane University, New Orleans, La.
Vaughan, Martha, M.D., Insulin. Senior Assistant Surgeon, Laboratory of Metabolism, National Heart Institute, Bethesda, Md.
Vellios, Frank, M.D., Biology. Assistant Professor, Clinical Pathology, Indiana University, Indianapolis, Ind.
Verge, Ernest, M.D., Surgery. Private Practice, Quebec, P.Q., Can.
Verge, Willie, M.D., Medicine. Hôpital l'Enfant-Jésus, Quebec, P.Q., Can.
Vestle, Charles Edmonde, M.D., Physiology to Surgery. Attending Surgeon, Neosho Memorial Hospital, Humboldt, Kans.
Vickers, Martyn A. M.D., Allergy. Allergist and Dermatologist, Bangor, Me.
Vigoreaux, Jose R., M.D., Biology, Psychology. Residency in Psychiatry, Santurce, Puerto Rico.
Vine, Sherwood, M.D., Gastroenterology. Private Practice, Trenton, N.J.
Vollman, R. F., M.D., Endocrinology. Resident, Obstetrics and Gynecology, Doctors Hospital, Milwaukee, Wis.
Wadlund, Robert Reed, M.D., Radiology. Assistant Professor, Radiology, Albany Medical College, Albany, N.Y.
Walker, Warren Franklin, Jr., Ph.D., Anatomy. Assoc. Professor, Zoology, Oberlin College, Oberlin, Ohio.
Wallaert, Albert A. J., M.D., Psychiatry. Chief, Psychiatric-Recorder's Court-Psychopathic Clinic, Detroit, Mich.

Weaver, J. Calvin, M.D., Anthropology. Private Practice, Atlanta, Ga.
Weinrebe, Joseph, M.D., Medicine. Private Practice, Boston, Mass.
Weiss, Arthur J., M.D., Hematology. Fellow, National Foundation for Infantile Paralysis, Philadelphia, Pa.
Weiss, Bernard W., M.D., Medicine. Private Practice, Yonkers, N.Y.
Whitehead, Leston S., M.D., Psychology. Private Practice, Detroit, Mich.
Windham, Samuel W., M.D., Medicine. Private Practice, Dothan, Ala.
Wineburgh, George, M.D., Medicine. Private Practice, Utica, N.Y.
Winkler, Gerald F., M.D., Internal Medicine. Senior Interne, Winnipeg General Hospital, Winnipeg, Manitoba, Can.
Zarowitz, Harold, M.D., Medicine. Private Practice, Brooklyn, N.Y.
Ziegler, Dewey K., M.D., Medicine. Montefiore Hospital, New York, N.Y.
Ziegler, Edwin E., M.D., Pathology. Director of Laboratories, St. Joseph's Hospital, Lancaster, Pa.
Ziferstein, Isidore, M.D., Psychiatry. Private Practice, Los Angeles, Calif.

STUDENT MEMBERSHIP

Geduldig, Michael Marc, B.S., Medicine. Student, New York University, Bellevue Medical Center, New York, N.Y.
Novak, Stanley, Psychology. Student, Hunter College, New York, N.Y.
Rowe, Irving, B.S., Physics. Student, Polytechnic Institute of Brooklyn, Brooklyn, N.Y.
Spitzer, Franklin, Biology. Student, City College of New York, New York, N.Y.